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CIBA FOUNDATION SYMPOSIUM
ON
MEDICAL BIOLOGY
AND
ETRUSCAN ORIGINS

Editors for the Ciba Foundation

G. E. W. WOLSTENHOLME, O.B.E., M.A., M.B., B.Ch.

and

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PREFACE

THE occasion of the fiftieth symposium to be held at the Ciba Foundation was marked by the choice of a subject outside the range of previous conferences. The Foundation's policy of promoting co-operation between workers in various branches of science was extended to bring together those in the humanities and in science; and "The recent contributions of medical biology to ethnology; with special reference to the origin of the Etruscans" provided a common basis for discussion of the interests of archaeology, anthropology, history, philology and human genetics.

The subject was originally suggested by Professor F. G. Young who had talked it over with Mr. R. M. Cook and Mr. G. S. Kirk at Cambridge. It was felt that a group with interests so diverse as archaeology and human genetics could best be integrated in a meeting at the Ciba Foundation, and the proposal received the full support of the Director. As a result, the conference, the proceedings of which are recorded in this volume, was held in April 1958 under the skilful chairmanship of Mr. Cook, when a concerted effort was made by the group to devise practical means by which the various disciplines represented could help one another in future research. The proceedings were held in both the English and Italian languages.

The Director warmly thanks Professor Young, Mr. Cook and Mr. Kirk for putting the idea forward originally and, together with Dr. A. E. Mourant, for helping with the organization of the symposium; Dr. M. Siniscalco for invaluable assistance with problems arising out of the use of the Italian language; Sir Gavin de Beer, Director of the British Museum (Natural History), and Dr. K. P. Oakley for the loan of skulls exhibited; Mr. D. E. L. Haynes, Keeper of Greek and Roman Antiquities, and his colleagues at the British Museum for kindly arranging a reception

and special display of Etruscan exhibits at the British Museum; and Dr. R. Guerrieri, of the Italian State Tourist Office, through whose courtesy complimentary copies of the review "Italy's Life" (1957, No. 24. Bergamo: Istituto Italiano d'Arti Grafiche) were presented to all members of the symposium. Special thanks are due to Mr. J. and Mr. J. A. Rivers of J. & A. Churchill Limited, who readily co-operated in the production of this unusual volume.

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Medical Biology and Etruscan Origins
G. E. W. Wolstenholme,Cecilia M. O'Conner
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INTRODUCTORY ADDRESS

R. M. Cook

Museum of Classical Archaeology, Cambridge

When I was invited to take the Chair at this symposium I was surprised, since I know little of Etruscology and less of biology. But I reflected that in Etruscan questions the specialists have often committed themselves to one answer or another, and that I had the advantage, if not of an open, at least of an empty mind. By such arguments I soon convinced myself that it was my duty to accept so flattering an offer.

The idea of using biological methods to determine the origins of the Etruscans occurred to Mr. Kirk and Prof. Young about two years ago, while they were talking after dinner in Trinity Hall at Cambridge. Next morning the idea still seemed good and, after some inquiries, Prof. Young put it to the Executive Council of the Ciba Foundation of which he is a member. Dr. Wolstenholme, the Director, cheerfully undertook the long and exhausting task of organizing a meeting, and now you must decide how and where the genetical connexions of the ancient Etruscans can be investigated most usefully.

The biological theory is roughly this. The population of any country or even province tends to have peculiar physical characteristics which were vulgarly called racial before "race" became a term of political propaganda, and are now better called "genetical". These genetical characteristics are remarkably uniform and probably persistent. They can be detected conveniently in the blood of living members of a population and, it is said, in the bones of dead members. It should, then, by taking enough samples be possible to show what is the physical relationship

between different populations, present and past. As the final five papers presented at this symposium will show, the study of genetics is fairly recent and still advancing vigorously.

The genetical examination of a living population is fairly straightforward; but any dead population presents special difficulties. There are two approaches, complementary rather than alternative. One is to take samples from the descendants of the dead population, and the other to test its bones. In taking samples from descendants it must first be decided who and where these descendants now are: there may at various times have been immigrations or fusions which altered the genetical character in some districts, and on this we need the advice of our historians. For bones we rely on the archaeologists, and primarily here our Italian colleagues, to provide them and to identify the culture and context to which they belong. By these means it may be possible to discover the genetical character of our dead population. If we wish also to discover the genetical connexions of this population, we must apply the same procedures in other relevant places. In the case of the Etruscans, who may or may not have been an immigrant population, this means (I suggest) that we must first compare them genetically with other adjacent populations of their time; and afterwards, if the comparison shows significant differences, with the populations of remoter places. The choice of such remoter places would certainly be difficult and is better deferred until we know what is necessary. There is one other, preliminary problem—to define, so far as we can at this stage, what we mean by "Etruscans".

The culture known as Etruscan becomes clear about the end of the 8th Century B.C. in the region between the Tiber and the Arno, the ancient Etruria; it spreads to the eastern basin of the Po and into Campania, and then gradually retreats and loses its special character, until by the beginning of the Christian era it may be pronounced extinct. Since it falls in a historical period, i.e. one from which useful written records survive, there can be

no doubt that the population which created this culture was that known to the Greeks as Tyrrhenian and to the Romans as Etruscan. But it does not follow automatically that all the inhabitants of Etruria were in the same sense Etruscan.

Interest in the Etruscans is old. The Romans, who were their neighbours and for a long time their inferiors in civilization, at first feared and learned from them. Later, when the Etruscans were no longer dangerous, they acquired an aura of venerable mystery, the more so since their language was no longer understood. With the Renaissance, historical curiosity revived, and was inflated by Tuscan opposition to Papal rule: it was comforting to remember that the greatness of Etruria was older than that of Rome. The fullest expression of what was later called "Etruscomania" was, as so often happens, the work of a foreign convert. Thomas Dempster was a Scot who became Professor of Law at Padua and there between 1616 and 1619 composed his *de Etruria Regali*, a monument of the widest reading and the most uncritical prejudice. Dempster's book was not published until 1723–26, and since by then the archaeological remains were taking first place with Etruscan students, F. Buonarroti, its editor, added numerous illustrations and a commentary on them. He was succeeded by A. F. Gori, among others, who "found something Etruscan everywhere", even in Homer. It is symptomatic of this new interest that painted Greek pottery, which was now being studied, received the name "Etruscan" although most specimens had been found in the Greek graves of South Italy. The scholars were soon forced to correct this error, but such is the attraction of Etruria that even now, 200 years later, Greek vases are still occasionally called Etruscan by the laity. During the later 18th Century enthusiasm for the Etruscans began to decline and in the 19th Century Greece attracted most students of classical antiquity. But the discoveries at Vulci and elsewhere after 1828 aroused a more historical interest in the Etruscans; and in 1848 George Dennis's *Cities and Cemeteries of Etruria* engaged

the attention of two generations of North Europeans. In our own time we have had a new breed of Etruscomaniacs, revolted by Greek perfection and finding in Etruscan art some curious psychological catharsis. It is, indeed, difficult to approach Etruscan problems with impartial detachment.

What the Etruscans achieved does not concern us here, and there are enough books already to instruct or delude us. Our problem is the origin of the Etruscans. On this, the evidence so far used comes from ancient literature and records, from linguistics, and from archaeological material.

Egyptian records of the 13th Century mention raids on their country from the sea. Among the raiders were a people whose name they spelt *Twrwš.w* and perhaps pronounced "Turusha". This may or may not be an equivalent of "Etrusci". The records do not say where the "Turusha" came from, nor do the names of their allies help in locating them.

To come to Greek literature, a passage in the Hesiodic *Theogony* (1011–16), which should not be much later than 700 B.C., speaks of Agrius and Latinus, ruling the Tyrrhenians in the holy islands. But the first important witness is Herodotus (i, 94), an Asiatic Greek active around 450 B.C. and justly honoured as the father of history. In his account of Lydia, the part of western Anatolia inland of Smyrna, he includes incidentally a story which he says was told by the Lydians. In the reign of Atys, which he put not later than the 13th Century B.C., Lydia was afflicted by famine; by inventing various games and other palliatives the Lydians endured for 18 years; then they drew lots and half of them sailed away to Italy under their prince, Tyrrhenus; from him they took the name "Tyrrhenians". Hellanicus, also writing in the 5th Century B.C., is said (in *Dion. Hal.*, i, 28) to have identified the Tyrrhenians as an immigrant branch of the Pelasgians, an elusive and ubiquitous people of Greek mythology. Anticlides, a historian of the 3rd Century B.C., is quoted by Strabo (v, 221) as bringing the Tyrrhenians from Lydia, together with some

Pelasgians of Lemnos and Imbros, islands opposite the entrance to the Dardanelles. Finally, Dionysius of Halicarnassus (i, 25–30), a Greek historian of Rome active in the later part of the 1st Century B.C., asserted that the Etruscans were natives of Italy: his long disquisition relies largely on mythology no more cogent than that of Herodotus, but contains the useful observation that—presumably in his time—the Lydian and Etruscan languages were not alike. What the Etruscans themselves thought about their origins is not recorded, and it does not seem likely that the answer will be found in the surviving Etruscan texts, if ever they are fully deciphered. But, in general, the opinion of ancient writers, Greek and Roman, followed Herodotus in supposing that the Etruscans came from Lydia.

The value of these ancient testimonies is not very great. For their accounts of prehistoric times, the Classical writers relied largely on legends, speculations based on popular etymology, material remains, analogy, and patriotic, political or personal imagination; nor did they usually state their sources and reasoning. So we cannot decide directly what truth may lie behind these various accounts of the origin of the Etruscans. It is, however, worth noting that the establishment of the Etruscans in Etruria was considered to go back to the legendary period of prehistory or, to make a rough conversion into modern terms, the late 2nd millennium B.C. The modern theories are concerned with a much later period.

A different kind of evidence, provided mainly by literary sources, is about the institutions and rituals of the Etruscans, some of which were borrowed or noted by the Romans. The Etruscan cities were sovereign states, participating rather than combining in a loose federation. Within each city there seems at first to have been some sort of monarchy, superseded in later times by an oligarchy of the aristocratic families. There is nothing exceptional about this. In the social life of the aristocracy, as wall-paintings also show, women had a fair degree of freedom. In this the

Etruscans differed markedly from their Greek and probably their Oriental contemporaries: Roman parallels may, of course, be imitative. The Etruscan rituals, in their elaborate and learned detail, appear by Greek standards Oriental, and their favourite method of divination—by examining the liver of the sacrificial victim—has its best parallels in Mesopotamia. This line of inquiry too leads to divergent results.

The linguistic evidence is more trustworthy, so far as it goes. We have a large number of Etruscan texts, mostly short and of obvious meaning, and Latin and Greek writers preserve and explain some Etruscan words. So we have some notion of the vocabulary and grammatical formations of Etruscan as far back as the 7th Century B.C. We have a fair knowledge of what languages were spoken in Italy at that time: none of them is related to Etruscan; nor is Greek, nor Lydian (as we know it). Indeed, the only close analogy—so it seems—is provided by a single and unintelligible inscription from Lemnos, probably of the early 6th Century. There is, of course, the chance that a language related to Etruscan may be discovered elsewhere, but it is unwise to reason on chances.

The archaeological evidence from Etruria is unusually rich, though one-sided, in that most of it comes from tombs. Fortunately the early Etruscan society, like other early societies, believed in equipping the dead with a lavishness proper to their station in life. So we have graves of varying elaboration, a very large quantity of pottery of the kinds used in Etruria, and many objects in metal. From these remains, archaeologists busily draw conclusions about the date at which the Etruscan culture appeared and the influences that affected their arts and crafts. It is difficult to be impartial in summarizing these conclusions.

First, it appears that in the 8th Century B.C., or perhaps earlier, there existed in and around Etruria an early Iron Age culture known as Villanovan. How widely this Villanovan culture extended is in part a matter of definition, but related pottery

and metalwork are found as far afield as southern Italy. It is generally regarded as the product of native Italian societies that had entered the Iron Age but not progressed much towards urban civilization.

Secondly, by the end of the 8th Century B.C. a much richer culture is found about the sites of the historical Etruscan cities. This Etruscan culture shows a much richer variety of grave goods, and in its arts the Villanovan tradition is improved and supplemented by Greek and Oriental (principally Syrian and Phoenician) examples and imports. To take an instance from pottery, the reduced ware known conventionally as Etruscan bucchero develops out of Villanovan, but the improved techniques probably and some of the shapes certainly are imitated from Greek. At first, Greek and Oriental influences are about equally visible, but during the 7th Century the Greek prevails and gradually even Greek deities and mythological figures are accepted into Etruscan art and belief.

Such, very roughly, are the data from which proceed most of the theories about the origins of the Etruscans themselves. These theories may be divided into two classes, one asserting that the Etruscans were immigrants and the other claiming them as natives or at least naturalized before the Etruscan culture began.

That the Etruscan culture was brought or developed by immigrants or invaders is the traditional view. It has to support it the sudden or rapid appearance of the Etruscan culture, its remarkable superiority over the other cultures of Italy outside the Greek colonies, its strong artistic connexions with the East Mediterranean, its elaborate and perhaps Oriental ritual, and its un-Italic language. But where these immigrants came from is not agreed. In the 19th Century some scholars brought them from North-East Italy; but those parts were culturally no more advanced than central Italy, and the northern theory is now justly discredited.

The arguments for an Oriental origin are more alluring. Most

of the higher phenomena of Etruscan culture can be paralleled in Greece and the Near East (as it used to be called before military geographers invented the "Middle East"); the language seems to be connected with that of contemporary Lemnos; and it is always gratifying to Classical loyalists to find Herodotus right, and, indeed, ancient tradition generally. But there are difficulties. If the Etruscans migrated much before the late 8th Century B.C., when the Etruscan culture appears, then that culture or its elements can hardly have been introduced by the Etruscans themselves and they might as well be regarded as natives of Italy. But if the migration of the Etruscans occurred in the later or even the mid 8th Century, it might be expected that Greek tradition would mention it in the right context, since by then the Greeks were frequenting and colonizing southern Italy. The Greek and Oriental components of Etruscan art might be due to trade, as were the Oriental components in Greek art of that time. As for the Villanovan components, it would be surprising if a higher culture, such as the Etruscans are supposed to have brought with them, borrowed so much from the inferior culture of their Italian neighbours. Besides, if the Etruscans came with an established and advanced way of life, they should not have been so susceptible to Greek influence, especially in the field of theology and myth. Lastly, there is no part of the eastern Mediterranean, admittedly not yet completely explored, which by the tests of language, customs and art can claim to be the original home of the emigrant Etruscans: Lemnos, to be sure, is linguistically possible, but in size it is too small to have peopled the early Etruscan cities. I pass over the arguments from the rite of burial—inhumation or cremation—since, despite the prehistorians, I doubt whether it has much significance: in Athens, for instance, without any apparent reason, the rite of burial changed four times between the 12th and the 6th Century B.C. There are other difficulties, too, about an Oriental origin of the Etruscans, but none is decisive. Most Classical archaeologists still believe that the

Etruscans came from the East, and probably from some part of Asia Minor.

The theory that the Etruscans were native, or rather that they were established long before the 8th Century B.C., has only recently received a logical basis. The argument is roughly this: before Indo-European languages reached the northern lands of the Mediterranean, i.e. before what we may call the Greeks and Italians and other peoples occupied Greece and Italy and parts of Asia Minor, that region was inhabited by peoples who spoke languages belonging to another linguistic family. As Indo-European advanced the older languages became restricted to a few isolated pockets—in Italy to Etruria, in the Aegean to Lemnos and perhaps to some other places from which no linguistic record survives. In the early Iron Age these Etruscans developed or adopted the Villanovan culture, and in the late 8th Century B.C. evolved from that into what we call the Etruscan culture. The causes of this evolution were contact with Greek and Oriental traders and economic prosperity: the necessities and luxuries of a higher, urban culture were grafted onto the native Villanovan tradition. The objections to this theory are again rather subjective. The rise of the Etruscan culture was too sudden to have been achieved by Villanovan peasants. It is not clear why the Etruscans alone of the native Italians should have made such progress. The elaborate rituals, where tradition is most tenacious, do not suggest a society of Villanovan standard. This theory of Etruscan origins is held most widely in Italy, where the archaeologists are most familiar with the Villanovan material. It is in some ways neater than the Oriental theory; in some ways—notably in the linguistic explanation—perhaps too neat.

Despite their more bigoted exponents, these two theories are not entirely exclusive. It is possible that the creation of the Etruscan culture was the effect of a small band of invaders who imposed themselves on Villanovan natives of lower culture, much as in England the Normans imposed themselves on the native

Anglo-Saxons. If the invaders were few, their departure from the East might have left no mark on ancient traditions, and they or a fair part of them might have come from a place as small as Lemnos. If the newcomers formed a ruling aristocracy in Etruria, importing pottery and metalwork, and perhaps some artisans from more civilized countries, and employing native craftsmen to imitate them, then some of the archaeological requirements would be satisfied. Those who wish to do so might reconcile other anomalies by supposing that the invaders were of mixed origin, though in that case one might expect greater differences between the cultures of the various Etruscan cities. A corollary of this explanation of the Etruscans proper as being a dominant minority is that the Etruscan language should be the language only of the invaders and that at the time of the invasion the population which they subjected spoke another, presumably Italic, language: perhaps the philologists can show whether or not this is possible, but I doubt if they have the evidence. I am not arguing here that this compromise theory is right, but since it is not impossible and its genetical effects are peculiar, I think that it must be taken into account when we discuss methods to be adopted in this inquiry.

The genetical consequences of these theories must be considered carefully. Let us assume provisionally that it is possible to identify descendants of the populations living in the 8th and 7th Centuries B.C., both in Etruria and in adjacent districts of Italy, and that we can obtain sufficient samples of bones not only from Etruria and adjacent districts, but also from the two types of graves in Etruria, the "Etruscan" and the Villanovan. Then first, if the Etruscans were wholly immigrant or were a homogeneous pocket of older inhabitants bordered by alien populations of Indo-European speech, the result would be the same, namely, that there would be a clear genetical distinction. The next step would be to find where else there occurred a genotype similar to the Etruscan—whether in other probable pockets of the early

Mediterranean population or in some other region in the eastern Mediterranean. Secondly, if the examination of bones should show that the occupants of the Villanovan graves of Etruria were different from the occupants of the "Etruscan" graves, but similar to the population of adjacent districts, that would be a strong argument for an invading minority, and again the next step would be to look for relatives elsewhere. Lastly, it might turn out that the population of Etruria was genetically uniform both within itself and with the adjacent populations: that would be delightfully disconcerting to the Etruscologists.

It may be objected that the origin of a people is unimportant; what is important is the formation of its cultural or national unity. From the historical, but not perhaps the biological, standpoint this is true. But students of antiquity have wasted so much time and temper on the origin of the Etruscans that it would be a notable service to scholarship to remove this cause of foolish dissension. I hope that our symposium will lead at least to this negative success.

Medical Biology and Etruscan Origins
G. E. W. Wolstenholme,Cecilia M. O'Conner
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THE PRINCIPAL CITIES OF SOUTHERN ETRURIA AND THEIR SPECIAL CHARACTERISTICS

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In a discussion of this subject, a brief outline must first be given of the geographical aspects of that district of the Italian peninsula which was known in ancient times as Etruria and occupied by the people who were known historically as Etruscans; because, for geographical as well as historical reasons, a distinction may be made between the cities of northern and of southern Etruria.

Etruria, which constituted the seventh Augustan district of Italy, lay between the rivers Tiber and Arno and the Tyrrhenian sea. This territory, which has altered little in character throughout history, is mountainous and hilly, softly undulating, sometimes with isolated peaks, indented by valleys through which rivers flow, with a coastline which is at times rocky, at times sandy and with some promontories. To the east, the ground rises from north to south following the line of the Apennines, and consists essentially of sandstone; the backbone of the mountains is hard stone of the types known as "serena" and "fetida" (in the district of Chiusi), with variations in grain and colour. The sandstone alternates with strata of scaly clay and clay schists which readily disintegrate, particularly where there is a lack of vegetation. Here and there different kinds of rock occur, e.g. serpentine and ofiolite.

Within the line of the Apennines extend the valleys of the great rivers, and the AntiApennine hills of Tuscany; here are

found the volcanic zones of southern Tuscany and northern Lazio, and the Tyrrhenian coast with its Maremma and promontories. The AntiApennines include the iron ore hills to the south of the river Cecina, where iron, lignite, galena, cassiterite and boric acid occur. Other characteristic features are the schist rocks of Mount Pisano and of the island of Elba, famous in antiquity for their iron ores; the various marbles to the west of Siena; the presence in other localities of lignite and of copper ores (in the Campigliese); chalk strata which have the appearance of alabaster (in the Volterrano); sulphur (in the valley of Staggia); and rock-salt (in the Volterrano).

With the exception of the region between Siena and Asciano, which is chalky, the whole AntiApennine complex is rich in vegetation, mainly olive trees, grape vines, cereals and fruit trees, and sometimes cypress trees. In ancient Etruria oak woods must have covered a vast area.

In the south stretches the volcanic zone of present-day Tuscany, dominated by two mountains, true volcanic cones, Cetona and Amiata, with volcanic rocks, gaseous and sulphurous emanations and thermal springs.

Further south is found the volcanic zone of Lazio which constitutes southern Etruria, and which is separated approximately from the rest of Etruria by the Fiora which flows into the Tyrrhenian sea, and the Paglia which joins the river Tiber. This region, which continues beyond the Tiber into the Alban mountains, includes three volcanic complexes which meet at Lake Bolsena (Volsini mountains), Lake Vico (Cimini mountains) and Lake Bracciano (Sabatini mountains). The volcanic manifestations here differ from those in southern Tuscany; instead of trachyte and lava, there is a predominance of tufa which forms sheer summits or peaks, emerging like islands, or plateaux separated from one another by deep gorges. The Tolfa mountains are trachytic, and are rich in minerals already exploited in ancient times, but they form an isolated and well defined complex.

The landscape of this largely tufaceous region is at times mountainous and wild, with dense vegetation consisting of great oaks and chestnut-groves, and then slopes gradually down to the banks of the Tiber, first through thickets, until it reaches the gently undulating grasslands of the Roman countryside.

Organized life was still more concentrated, from earliest times, in these volcanic regions than in the north; primitive villages united in towns, either near the sea or leading to it, and extended their territories until they became true states. They were situated relatively close to one another. At least five of the known twelve cities, Tarquinia, Vulci, Cerveteri, Veio and Volsinii, flourished in southern Etruria. As is well known, these were the most important in Etruria and developed the most rapidly, because of their nearness to the sea, and they decayed just as rapidly in the final phase of Etruscan civilization when Rome extended its domination over their territories and when the dreadful scourge of malaria swept through the Maremma. Nevertheless, the memory of their power, handed on by history, and the limited development of the region during the Middle Ages and in modern times contributed to the preservation of their remains, which still today bear witness to their ancient splendour.

Taking as a point of departure the region confined within the limits of present-day Lazio and Tuscany, I shall describe the cities of southern Etruria and the ancient territory of their states.

The state of Vulci was the most extensive, comprising the stretch of coast between the Uccellina mountains and the source of the Arrone, with a vast hinterland which embraced the valleys of the Albegna and the Fiora and reached as far as the western bank of Lake Bolsena. It was confined to the north by the territory of Roselle, to the east by that of Chiusi, along the upper reaches of the Paglia, and by that of Volsinii; and to the south by the territory of Tarquinia, along the course of the Arrone. Its capital, *Vulci*, was situated about ten miles from the sea between Montalto di Castro and Canino on a plain of about 150 hectares,

almost vertically above the river Fiora to the south and east, and bounded on the north by a fosse. The site, which today is known as "Pian di Voce", has only recently been identified as that of the ancient city; and still more recently, in 1956, systematic excavation was begun there, following years of careful exploration. Such scientific investigations have revealed the walls of the city, the gates, the internal roads and the houses. These discoveries relate to works dating from the 4th Century B.C. (walls), up to the time of Imperial Rome (roads, buildings, votive objects) but here and there finds have been made which are of an earlier date (6th–5th Century B.C.), e.g. terra-cotta of temple revetments, ceramics, etc. This allows us to hope that we may yet discover Etruscan remains of monuments from the period of greatest splendour of the city, which will make a notable contribution to Etruscology.

Already in the past the vast necropolises of Vulci were well known and yielded their treasures to the various museums of the world. Besides the earlier sporadic discoveries, from the beginning of the 19th Century extensive excavations of the tombs of Vulci led by Lucien Bonaparte, Prince of Canino, in the tufa around the ancient city, yielded to the studious, and still more to the acquisitive, innumerable specimens of Greek and Etruscan pottery, finely worked bronzes, and sculpture in nenfro and peperino. But their aim was not scientific; they hoped and professed that they knew the Etruscans and their civilization, while despoiling their vast cemeteries. Then in 1857 François and Noël des Vergers discovered the "François" tomb with its now famous pictures. The French School, under Gsell, initiated the first systematic excavations in the district of Cavalupo, and the Italians, Bendinelli, Mengarelli and Ferraguti, explored the region of the rock tombs of "Ponte Rotto" and the necropolises of Cavalupo and Osteria. The material, arranged and classified, constitutes a good complex from which it is possible to reconstruct the life of the city from the 9th to the 2nd Century B.C. Thus,

we know for instance—and this has been confirmed by the explorations of 1951–55 which prepared the ground for the latest excavations—that in the most archaic period, in the so-called "Villanovan" and in the "transitional" phases, Vulci consisted of a number of villages. Various necropolises of the 9th–7th Century B.C., with trench and well tombs, have confirmed this, and the equipment discovered at more recent burial sites has furthermore indicated a certain continuity between the culture of the first Iron Age and the Orientalizing period. Elements of both periods are mixed and fused together, giving an individual character to a production which embraces all of the first half of the 7th Century B.C. Perhaps these were the most interesting discoveries, confirmed by findings at other sites in Etruria which are next discussed.

The desolate character of the site of ancient Vulci, the flat and wild landscape only partly altered in recent years by the agrarian transformation of the zone, has always attracted the visitor who ventures into the Maremma and who all at once discovers the remains of the splendour of an ancient civilization; noting the hypogean tombs excavated on the left bank of the Fiora; *la cuccumella*, a strange tumulus still little studied; the old Roman bridge of Abbadia (Fig. 1), in use up to a late date as an aqueduct and resting perhaps on the tufa pillars of a primitive Etruscan bridge. All these monuments, together with the few Roman remains which are found on the plain and the mediaeval castle situated on the left bank of the Fiora, form a suggestive and interesting complex. From the 6th Century B.C., when it was at the height of its power due to sea traffic and perhaps also to the mineral industries of nearby Mount Amiata, Vulci was gradually forced to relinquish the numerous centres in the hinterland which had flourished, in part, in the Villanovan and Orientalizing periods. Among these were: *Suana* (Sovana), *Statonia* (identifiable with Poggio Buco or with Castro) and *Maternum* (Farnese), along the valley of the Fiora; *Aurinia* (later known as *Saturnia*),

FIG. 1. Vulci. Bridge and Castle of Abbadia.

FIG. 2. Norchia. Rock tombs.

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FIG. 3. Barbarano. Rock tombs of San Giuliano.

FIG. 4. Cerveteri. Tumuli.

Heba (Magliano) and *Caletra* (Marsiliana), along the valley of the Albegna; and, on the Tyrrhenian coast, *Cosa*, a port and Roman colony built beside an ancient Etruscan centre; and *Talamone*, another port in the extreme north of the territory of Vulci.

Near the territory of Vulci, which formed its north-western border, lay the state of Volsinii. This was bounded on the north by the Paglia, beyond which was the territory of Chiusi, and on the east by the middle course of the Tiber as far as Orte, which linked it to the district of Umbria, on the south-west by the territory of Tarquinia as far as Viterbo, and on the south by the Faliscan territory from which it was separated by the Cimini mountains. In this state arose one of the most important Etruscan cities, regarded as the capital of all the Confederation, viz. *Volsinii* (in Etruscan, *Velzna*), in the neighbourhood of which was the famous *Fanum Voltumnae* where the annual assemblies of the twelve Etruscan *populi* took place, and the site of which has not yet been identified.

Recent excavations, undertaken since 1946 by the French School in Rome in agreement with the Italian Government and under the direction of Professor Raymond Bloch, and which are still in progress, have shown with certainty that the site of ancient Volsinii corresponds to that of modern Bolsena and not to Orvieto as was formerly thought. A huge double enclosure of walls made of square blocks of tufa surrounded the city. A long stretch of this was uncovered in 1947 and, in 1957, another short stretch was uncovered, perfectly preserved, in the interior of the modern town. Scientific exploration of the site led to the identification of numerous Etruscan tombs (chamber and trench), and to the discovery, south of the city, at the site known as *La Cività*, of a *pagus* with remains of dwellings and of an archaic necropolis dating from the end of the 8th to the beginning of the 7th Century B.C., which yielded many personal effects from the late Villanovan period. Was this, as at Vulci, one of the villages which in more recent times constituted the actual town itself? It

seems to be so. Then we must also consider the possibility of an evolution of the pre-existing populations in the territory rather than a replacement of them by new populations which came from the sea in historical times. The hypothesis is suggestive and merits careful study.

Even at the present time, one of the chief archaeological centres of the region of Volsinii is Orvieto, with its necropolis, painted tombs and temple; situated on a spur of tufa, tall and majestic, it dominates the valley of the Tiber. It could be identified with ancient *Salpinum* which, according to Livy, was a powerful and autonomous centre near Volsinii. Other centres of the region were *Ferentium* (Ferento), *Polimartium* (Bomarzo) and *Horta* (Orte).

On the Tyrrhenian coast, extending from the mouth of the river Arrone to that of the Mignone, was the territory of Tarquinia, the "mother city" of Etruria, with a hinterland comprising the region between Lake Bolsena and Lake Bracciano. It was bounded on the north by the territory of Vulci and the south side of Lake Bolsena; to the east by the territories of Volsinii, Falerii and Veii, along a line which has not been determined with certainty; to the south by the territory of Caere, from which it was separated by the Mignone and by the trachytic Tolfa mountains.

The site of ancient *Tarquinii* [in Etruscan *Tarh(u)na*] is now known, following the endless controversies of the past, as a result of the excavations in 1934–38 on the hill, *La Cività*, which like the other sites of Etruscan centres is elevated and isolated. Remains of the imposing circle of walls have been brought to light, together with the foundations of the great Etruscan temple called the *Ara della Regina*; the ornamentation of this temple included the group of winged horses in terra-cotta which is considered to be one of the great masterpieces of Etruscan art.

Excavation of the site is still being carried out enthusiastically; quite recently work was resumed within the area of the ancient

city, and in the neighbouring zones occupied by the necropolises (i.e. the hills of Monterozzi and Cavalluccio), as well as the area of the mediaeval and modern city. Beneath the modern town, in the vicinity of the Via Aurelia, a small cemetery was discovered and completely excavated about two years ago. Dating from the first Iron Age, this provides a very good illustration of the Tarquinian culture of the 9th–8th Century B.C. Among the most recent discoveries is that of a painted tomb of the end of the 6th Century B.C., found in the necropolis of Monterozzi, in addition to the thirty or so tombs already known. It has not yet been excavated, but was detected by making soundings, and the interior has been completely photographed with a small camera.

The necropolis and painted tombs constitute the most interesting feature of the archaeological zone of Tarquinia. Here we can form a very clear idea of Etruscan painting, which is a reflection of the lost Greek art, from the middle of the 6th to between the 2nd and the 1st Century B.C. Chamber, trench and cremation tombs are also found. A visitor may think that the tombs are isolated and far away from one another, since only the painted ones are opened and accessible; but, in fact, for some 5 km. the whole hill of Monterozzi is occupied by sepulchres.

The state of Tarquinia included on the coast the port of *Graviscae* (Porto Clementino) and the landing places of *Martanum*, at the mouth of the Marta, and *Rapinum* at the mouth of the Mignone; inland, besides *Tuscania*, which flourished during the 4th Century B.C. and afterwards, another town which probably belonged to the state was *Visentium*, on Lake Bolsena. The necropolis of Visentium was excavated systematically from the end of the 19th Century onwards, the most recent excavation having been carried out in 1957. It has yielded equipment from various kinds of tombs, trench, well, trench with nenfro interior, only rarely of the chamber type, dating from the end of the 8th and the whole of the 6th Century B.C., and of particular interest for the study of the archaic cultures of Etruria. The numerous

bronze objects found here suggest that Visentium was influenced by Vulci. The problem can only be solved by continued research.

That part of Tarquinia distinguished by rock tombs should be considered separately. Castel d'Asso (perhaps the ancient *Axia*), Norchia (ancient *Orcla*) (Fig. 2), Blera and San Giuliano (*Marturanum* or *Manturanum*) (Fig. 3), arose in a region with its own unmistakable character. It consists of a series of tufa hills where the tributaries (on the left side) of the Marta arise, e.g. the Biedano and the Leia, and on whose rocky summits there dwelt small communities in true *oppida*. All around, on the slopes of the hills, are the necropolises with rock tombs sculptured in imitation of architectural structure and decoration. In ancient times they must have constituted a remarkable and characteristic unit; also today the sight of those sculptured walls, those tombs like dice rising among the thickets of a still wild and primitive region, delight the visitor who ventures into the valleys and hollows "in search of the Etruscans".

Among the states of southern Etruria, that of Caere covered a long stretch of the coast and, therefore, was the one most closely linked to the sea. It extended from the Mignone, which separated it from the state of Tarquinia, to the southern course of the Arrone. Bounded on the east by the territory of Veii, it included most of Lake Bracciano and, to the north, the Tolfa mountains. The capital, *Caere* (known to the Greeks as *Agylla*), was situated about 6 km. from the sea and occupied three parallel tufa hills, separated from one another by the Manganello and the Mola. On the central hills was also the urban centre, extending for 150 hectares, with an almost rectangular perimeter. In the past, this area has yielded, besides Roman remains, the beautiful series of Imperial statues now at the Lateran Museum, the architectural remains and terra-cottas of many temples. All around the town were vast cemeteries (Fig. 4): those of the first Iron Age (Cava della Pozzolana e Sorbo), with trench and well tombs, were the nearest to the town; those of the 7th–2nd Centuries, B.C.

occupied the two parallel hills of the Banditaccia and of Mount Abetone, with the exception of the famous Regolini-Galassi tomb of the Orientalizing period which is situated near the Sorbo. The necropolis of the Banditaccia, plundered in the 19th Century, has been largely excavated systematically from 1911 to the present time. Hundreds of chamber tombs, sepulchral roads and tumuli are now restored and accessible, and form a grandiose monumental complex unique in the whole of Etruria. The tumuli in the region so far explored, some of which are 30 m. in diameter, are numerous; inside are groups of tombs hewn out of the tufa mass in imitation of the interior of dwellings, reproduced exactly in every architectural detail. Here we have the most important evidence of Etruscan architecture.

These tombs have yielded a great amount of archaeological material, especially vases, both Etruscan and Greek, representing all periods.

As shown by the abundance of material imported by sea from the eastern Mediterranean and from Greece, Caere—although at a distance of some kilometres from the sea—could be considered a great coastal town; its ports on the Tyrrhenian sea were *Alsium* (near Palo), *Punicum* (Santa Marinella) and *Pyrgi* (Santa Severa). No important remains of the first two have been found, and until 1957 all that was known from the third were some Roman remains and the walls of the *castrum*. A very important discovery made in 1957 revealed the imposing remains of the Etruscan town Pyrgi from the beginning of the 5th Century B.C. Excavations undertaken by the University of Rome in collaboration with the Soprintendenza alle Antichità dell' Etruria Meridionale brought to light the remains of a temple with its foundations of tufa blocks, and numerous fragments of architectural terra-cotta of the second phase, some of which seem to have been part of the decoration of a tympanum. They include statues or fragments of statues in polychrome terra-cotta, two-thirds life size, representing Athena and perhaps the Giants. Could this be the Sanctuary of

Leucothea, remembered in tradition and destroyed by Dionysius of Syracuse in 384 B.C.? Everything suggests that it is so; and it is confirmed by the discovery of two fragments of vases inscribed with dedications to *Uni*. As already mentioned, the territory included the Tolfa mountains and the towns of Tolfa and Allumiere, as well as centres which should be studied and explored in order to throw light on the archaic cultures of this region and on the eventual exploitation in ancient times of its metal mines.

The territory of Veio, the nearest to Rome, stretched as far as the sea to the south-west, between the mouth of the Tiber and the southern Arrone, and was bounded by the territory of Caere and by Latium. A belt of land extended to the north between Lake Vico and Lake Bracciano, confined by the territories of Tarquinia to the west and Falerii and Capena to the east.

Following the period of Veio's supremacy over Rome (in the 7th and 6th Centuries B.C.) came the beginning of hostilities between the two towns, and the territory suffered its first mutilations with the occupation of the coast by the Romans. When the town was conquered by Rome in the year 396 B.C., the territory was annexed to the Roman state.

A few kilometres from Rome, the capital, Veio (*Veii*), arose at the confluence of the two branches of the Cremera. The site was identified at the beginning of the 19th Century, and today is one of the most suggestive of those in the Roman countryside, on the right bank of the Tiber; a desolate place, perhaps, but appearing as a fantastic complex of tufa rocks, dense vegetation and waterfalls.

The acropolis was located in the *Piazza d'Armi* where, among other archaeological evidence, were found the remains of an archaic temple of the first period. In the region of Portonaccio, in 1916, were discovered the remains of the foundations of the temple of Apollo whence came the famous statues now in the Villa Giulia Museum in Rome, and which have been attributed

to a school of artists or to one artist named Vulca who, in about the year 500 B.C., also worked in Rome in the Temple of Jupiter Capitolinus.

The necropolis extended all around the plain and, although still little excavated, has yielded a considerable amount of equipment of the first Iron Age. A chamber tomb of the Orientalizing period, the *Campana*, contains the most ancient known Etruscan pictures, from the beginning of the 6th Century B.C.

In recent years some systematic excavation has been carried out in the archaeological zone of Veio, yielding many fragments of statues from the Temple of Apollo. The territory of Veio includes Sutri, which has tombs with architecturally sculptured façades; and Nepi, near the Faliscan border. Few other archaeological centres are known. A study of the road system of the whole territory is being made by the British School at Rome.

It is hoped that this brief summary of the physical and geographical characteristics of the region under study, given from the archaeological point of view, may be of interest to the biologists and other scientists whom we archaeologists are glad to welcome as collaborators in research into a problem which is near to our hearts and which we would like to see resolved.

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ETRUSCANS AND UMBRIANS

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IN his opening address, Mr. Cook summarized with great clarity the highly debatable question of the origins of the Etruscans, which is the theme of our work and to the solution of which the scholars present here will surely make an important contribution, by reporting results of their research or by examining particular aspects of the history and of the civilization of the Etruscans. I have never been particularly interested in the origins of the Etruscan people or in the related problems, but my activity for many years in the territory of southern Etruria and in Umbria, an activity which has given me many possibilities to consider the methods of research employed in the study of the ancient Italic peoples, qualifies me to contribute to this symposium which is attended by some distinguished Etruscologists.

I shall indicate here the opportunity for a wider and more precise study of Italic peoples—particularly those who, like the Umbrians, had long-lasting and varied relations with the Etruscans—and the necessity for a critical revision of the problem of the formation and development of that part of Etruria in the region of the Tiber, namely the two towns, Orvieto and Perugia, and the contrasting behaviour of towns situated on the other bank of the river. This approach is directed not only towards a greater knowledge of the history and culture of the two peoples who, when already permanently settled on the two opposite banks of the river, constituted two well-defined political entities; but also to attempt to establish the nature and distribution of the very ancient populations of central Italy; to disclose what

is unknown about that primitive ethnic substratum—perhaps common to Etruscans, Umbrians and other peoples—the existence of which is assumed. It is also possible that this investigation will lead to a clarification of the question of the origins of the Etruscans or, at least, to a clearer perspective and a more exact evaluation of the Etruscan phenomenon, which is surely the most important point.

Strabo (v, p. 216), discussing the relationships between Etruscans and Umbrians, says that these two peoples, before the decisive intervention by the Romans, disputed the primacy of the territory, and frequently crossed the Tiber, which divided them, to fight one another. Recently, Pallottino (1954, p. 41) suggested that the dual role of the Tiber, which, like all rivers, sometimes separates but often also unites, could account for the complex relationships between Etruscans and Umbrians. It is certain that the Etruscans, with their more evolved civilization, exerted a very great cultural influence on neighbouring populations. The archaeological evidence of the finds in Todi, Bettona and other localities near the border, shows that in the Umbrian territory there was—if not political control in some towns, as was supposed by Becatti (1936, p. 129) for Todi—surely a cultural infiltration, which extended also to the interior (in the direction of the Sabina) as far as Monteleone di Spoleto, where evidence that cultural unity was achieved is provided by the carriage found there. On the other hand, one cannot exclude the possibility that the Etruscans assimilated something from the Italic people on the other bank, in view of the constant commercial exchange across the Tiber. This hypothesis, which must be carefully evaluated, is supported by the occurrence of Italic names in the sepulchral inscriptions of Orvieto, and perhaps by the different behaviour of the two great Etruscan towns of the Tiber region as compared to the sister towns of maritime Etruria; in particular it is supported, in my opinion, by the unification of Umbrian and Etruscan culture in Perugia, in the 3rd Century, a unification

which presupposes profitable contacts between the two peoples. How else could one explain the flowering of Perugia between the 3rd and 1st Centuries B.C.—where, even following the spread of Roman influence, there existed in full an Etruscan culture—or its revival after the Perusinum war? This revival occurred in Roman times in all of the numerous towns along the left bank of the Tiber—whose prosperity contrasts with what is found at the same period in the more truly Etruscan territory—as far as the Tyrrhenian Sea and the gates of Rome, a prosperity which evidently is related to the vitality of the Umbrians, and to their receptive character (Romanelli, 1954, p. 244).

So far, in discussing the Umbrians I have referred only to the inhabitants of ancient historical Umbria on the left bank of the Tiber, facing the Etruscan region of the Tiber; but now we turn to the question of who and whence were those populations which were to take part in a long and complex dialogue with the Etruscans. According to Strabo (v, p. 214), the Umbrian Italics were called in by the previous inhabitants of Greek origin, to help in resisting the incursions by the Etruscans. Apart from Strabo's testimony, which is debatable, it is not certain whether the name "Umbrian" relates to the historical Italic-speaking people who penetrated into central Italy, with successive migrations towards the west, or whether it relates to people of the more ancient stratum of the pre-Indo-European populations. Furthermore, supposing the existence of such a homogeneous pre-Indo-European stratum were established, we still could suggest an explanation of the formation of the two different nations, Umbrian and Etruscan, on the grounds that the future Etruscans were more faithful to the pre-Indo-European traditions, while the future Umbrians, having picked up the new influences, tended towards the Indo-European.

The names of certain towns, neither Italic nor pre-Indo-European, found in Umbria on the left bank of the Tiber, such as *Ameria*, *Spoletium*, *Hispellium* and *Assisium*, can be regarded as

belonging to the primitive ethnic stratum. Moreover, the same root of denomination chosen by the historical Italic Umbrians, who spoke a European language, reappears in the name of the river Ombrone in Tuscany and in that of some Etruscan families. As regards the more advanced period of prehistory, there are indications that an affinity was maintained for a long time—up to the beginning of historical times—between the populations on both sides of the Tiber. Nevertheless, the evidence so far provided by prehistoric archaeology, the toponomastic indications which are often deceptive, are not sufficient, according to Pallottino (1954, p. 42), for the reconstruction of a true picture of the distribution of populations in the Umbrian territory before the end of the Bronze Age, and of the evolution of the ethnic composition of central Italy, as indicated by the movements of the populations speaking Indo-European languages.

New investigations of some parts of Umbria could prove fruitful for study of the history of the Italic civilization. Such investigations should include, firstly, a critical examination and comparison of the known data and, secondly, new excavations carried out in accordance with modern concepts of stratigraphy. Devoto (1951, p. 83) has emphasized the importance of finds in various localities near Terni, and particularly near the steel factories; showing that, as a result of the excavations of the necropolis, Terni provides a better illustration than Bologna of the vicissitudes of the proto-Italic civilization, from the very archaic to the subsequent, but still pre-Roman, phases. Traces of some forty neo-Eneolithic huts, found under the tombs, show that another population preceded the Italics. With time, the invaders adapted themselves to the local funeral rites and, thus, although the funeral rite of incineration was retained the tombs became bigger and finally there emerged the tumulus surrounded by a circle of stones. Here, for the first time in a place which is certainly Italic, is found this custom which dates from antiquity and is seen to develop further in Etruria.

Other interesting finds have been made, and still may be made, in the territory of Foligno and in the region between Campi, Norcia, Cascia and Monteleone di Spoleto. The plateau of Norcia, according to Devoto (1951, p. 101), has not yet yielded up its most important material.

It appears, from the amount of data already available, that positive results may be expected from systematic topographic studies of Umbria, on both sides of the Tiber. It is to be hoped that, in the future, new research will be undertaken, together with the work of revision, to determine the characteristics and the history of a precise region. Since history is known through the synthesis and comparison of the results of different disciplines, we archaeologists are very happy to have with us biologists and other scientists who share in our effort to interpret the Etrusco-Italic world, in the certainty that the comparison of data from archaeology and glottology with those from the biological and naturalistic sciences will give more concrete results than have been obtained so far.

Therefore, it gives me great pleasure to express the wish that recent discussions, and particularly this meeting of archaeologists with other scientists, may mark the beginning of a new period of study and research leading to the solution of the difficult problem of the Etruscans and of the peoples of ancient Italy.

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ARCHAEOLOGICAL EVIDENCE FOR THE ORIGIN OF THE ETRUSCANS

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IN the ancient Mediterranean there existed in the past two distinct types of colonization. One was the Greek type. Greek colonies resembled modern colonies of European nations in that the Greek colonists represented only one cultural group and on the whole kept themselves apart from the natives. They were and remained Greek.

The prehistoric pattern in Mediterranean lands was quite different. Here the colonists often did not hold themselves so aloof from the older natives, and fresh waves of colonists kept arriving from the eastern Mediterranean and mixing with the older ones. Even the original Neolithic settlers bringing the first agriculture and domestic animals often adopted the Microlithic tools of the Mesolithic hunters among whom they settled.

In southern Italy and Sicily they are followed by more advanced Neolithic peoples making painted pottery, but one does not everywhere succeed the other directly. There is often a greater or a lesser continuation of the old along with the new. Even the makers of the painted pottery cannot be traced very precisely to their parent culture. They have marked resemblances to the Neolithic cultures with painted pottery of the Aegean and the southern Balkans (Bernabò Brea, 1957). But there is no complete identity. One may suggest that the colonists were already mixed when they arrived. Later on in the Copper Age and Early Bronze Age the emphasis shifts toward the Aegean and

Anatolia. But there is no such identity as there is between the various kinds of pottery in the Greek colonies in Magna Graecia and in Greece itself. There are, of course, some purely local movements like the intrusion of the Apennine people in Lipari or the Milazzo urnfield in Sicily. There are also some frankly imported Mycenaean vases. But the evidence of colonists from the eastern Mediterranean is less precise than that which identifies the Greeks at Syracuse or Cumae. Still a study of the material in southern Italy and Sicily leaves little room for doubt that such newcomers actually arrived in the central Mediterranean.

If one looks at Sardinia or Spain, the same is true but to a more marked degree. In Spain both the Almerian Neolithic and its successor, the Los Millares Culture with cupola graves and plenty of copper, were certainly composed of eastern Mediterranean elements, but with no clearly localized homeland (Childe, 1948; Leisner and Leisner, 1943, 1956). The succeeding Argar Bronze Age is often thought to have Anatolian affinities, but enough older features survive so that a continuation of the previous culture is also arguable (Bosch-Gimpera, 1954).

One could multiply many times these instances of early Mediterranean colonists whose homelands are vaguely defined or not defined at all. One has only to think of Malta as another example (Evans, 1953, p. 41 *f*). But it is indeed a strange thing that we know so much about so many early cultures all over the Mediterranean, but so little about their precise relationship to their homelands. It may be said that this is due to gaps in our knowledge. Gaps there undoubtedly are, but I would suggest that the reason why our knowledge of these relationships is confused is a reflection of confused situations. A given group of colonists did not come merely from one clearly defined area or culture, and also they mixed with the previous inhabitants.

To apply this to Etruria, I would suggest that, if eastern Mediterranean newcomers brought an Orientalizing civilization there, they were the last colonists in the old pattern. One can

trace individual items of their culture to various places in the eastern Mediterranean. But the eastern Mediterranean has not yet produced an Etruscan homeland complete with culture and language. Greek colonization was on quite another pattern.

A partial analogy to all this can be seen in America, a country that is European in its origin and with very large continental elements but, despite its use of the English language, is widely different from both England and continental Europe. The European population of America went there from many countries mainly for political, religious or economic reasons and a large segment was brought in slavery. Perhaps these early colonies in the Mediterranean had similar motivations and mixed backgrounds.

What I have said so far about prehistoric colonization is of course purely theoretical. Since we are dealing with the prehistoric past, we have no written words to guide us but only the archaeological material and our interpretation of it. But if in prehistoric times eastern Mediterranean traits of culture are found scattered widely in the central and western Mediterranean all the way to Spain, they surely did not come there by themselves. They must have been brought by eastern Mediterranean people.

The evidence of physical anthropology also tells the same story. According to Coon (1939, pp. 78 *f.*, 131 *f.*) these people were different from their older Palaeolithic and Mesolithic predecessors and belong to Near Eastern types of men. Coon also finds that the Etruscans were typical of the Bronze Age population in the eastern Mediterranean. Indeed, he says that they resembled physically the El Argar people of the Spanish Bronze Age who are also supposed to be in part eastern Mediterranean settlers (Coon, 1939, p. 154 *f.*).

So much for generalities. It is the view of Pallottino that the Etruscans were the result of a long and complex development in Etruria. The Eneolithic and Apennine Bronze Age people would have come in part at least from across the Adriatic (Pallottino,

1957*a*, pp. 36–37, 43, 51–52; Säflund, 1957, pp. 11, 28). The next step was the formation of the cremating Villanovan culture of the Iron Age, which for Pallottino arose out of the older elements in Etruria with the addition of a nucleus of cremating people from central Europe. Finally these people passed into the Orientalizing phase properly called Etruscan without any further infusions of outside settlers (Pallottino, 1957*a*, p. 90).

I would differ from this viewpoint only in bringing a larger number of cremating settlers in the Iron Age and at least a small but powerful group of eastern Mediterranean people at the beginning of the Orientalizing period.

Etruria was a poor and marginal country in the Eneolithic and in the Apennine Bronze Age (Pallottino, 1957*a*, p. 85; Puglisi, 1954), and a scattered pastoral population lived in caves and small villages. But with the beginning of the Villanovan Iron Age the scene suddenly changes (Säflund, 1957, p. 10*f*), and since these Iron Age people were the immediate forerunners of the Etruscans of history, it is necessary to enquire into who they were. Towns now appear on new and easily defensible sites, and these later became the Etruscan cities of historical times. At Tarquinia, Villanovan sherds are scattered over almost the whole area of the classical city and suggest a very considerable Iron Age town. The Villanovans also differed from the people of the Bronze Age in important features of their pottery, in a more developed metallurgy including iron, in a quite different armament industry and a new ceremonial life with a new mode of burial. Their most striking features are the enormous cremation cemeteries around their towns, for these indicate a large, settled and organized population. Graves of previous periods are not only much rarer, but they are always inhumations.

Now cremation cemeteries or urnfields first sprang up in Europe in Hungary in the Early Bronze Age and continued there through the Middle and Late Bronze Ages. In the Late Bronze Age the urnfields spread over large areas of Europe including the

pre-Villanovan ones in Italy. But the true Villanovan urnfields are characterized by a very special type of two-storied urn (Fig. 1 A).

Two-storied pots are scattered in the urnfield cemeteries in a wide arc from the Palatinate, Franconia and Saxony through

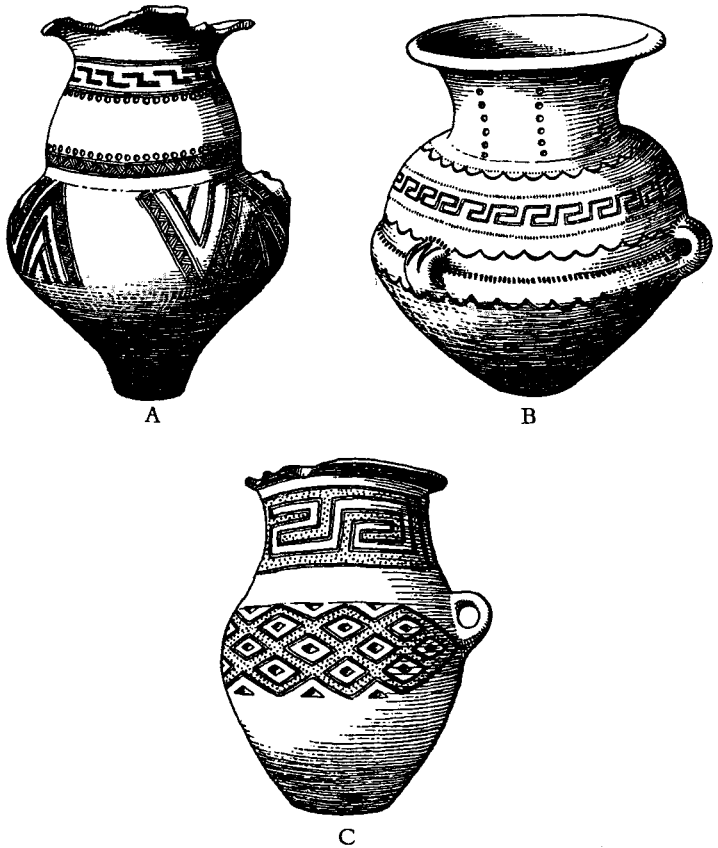

FIG. 1.
A Urn from Selciatello 53, Tarquinia.
B Urn from Ostrovul Mare, Rumania (after Berciu and Comșa, 1956).
C Pot from the Grotta Lattaia, Monte Cetona (after Pallottino, 1947).

Bohemia (Böhm, 1937; Childe, 1929), Hungary (Childe, 1950, Fig. 179) and the Balkans, but Macedonian examples are less convincing (Heurtley, 1925–26). The urns that are most like Villanovan are the Dubovac-Žuto brdo and Gîrla Mare urns found in the region where Hungary, Rumania and Jugoslavia come together (Berciu and Comşa, 1956, Fig. 40; Wosinsky, 1904, Plates LXIX, XCI, XCVI; Zambotti, 1954, Plates XIX, 110, 111; XXI, 128; Benac, 1956—see also for references). These are placed in the Middle and Late Bronze Ages and hence are quite old enough to be the ancestors of the Villanovan urn.* Sometimes they even have incised meander patterns (Fig. 1 B). These two-storied urns in eastern Europe seem to me to be more like Villanovan urns in shape than the Apennine Bronze Age pots to which Pallottino (1947, Plate VI) has pointed as their ancestors (Fig. 1 C). Still the meander patterns around the necks of the Apennine and Villanovan pots suggest a mixing of traditions. Pallottino at the same time agrees that there were some cremating settlers from central Europe (Pallottino, 1957*a*, p. 91).

Another element from eastern central Europe is the bell helmet (Fig. 2) (Merhart, 1941, p. 14, Map 2. If Merhart had shown the numerous pottery replicas for funeral purposes found in Etruria, the type would be much better represented there.). The lunate Villanovan razors were derived from a somewhat similarly shaped knife (Vogt, 1949–50, Figs, 1, 2, 4; Merhart, 1942; Foltiny, 1955, Plates LXVIII, LXIX) found in Switzerland, Czechoslovakia and Hungary in the urnfield period. This same knife also comes into Italy with the pre-Villanovan urnfields, as at Fontanella (Fig. 3). But the forerunners of the crested helmets (Fig. 4) have a more westerly distribution, mainly in Germany and France (Merhart, 1941, p. 26, Map 3. Again if the pottery

* I omit here the two-storied urns from the Tarxien urnfield in Malta, which are evidently the oldest of all, but their connexion with the others is at present hard to define (Evans, 1953, Fig. 10, 83, p. 86).

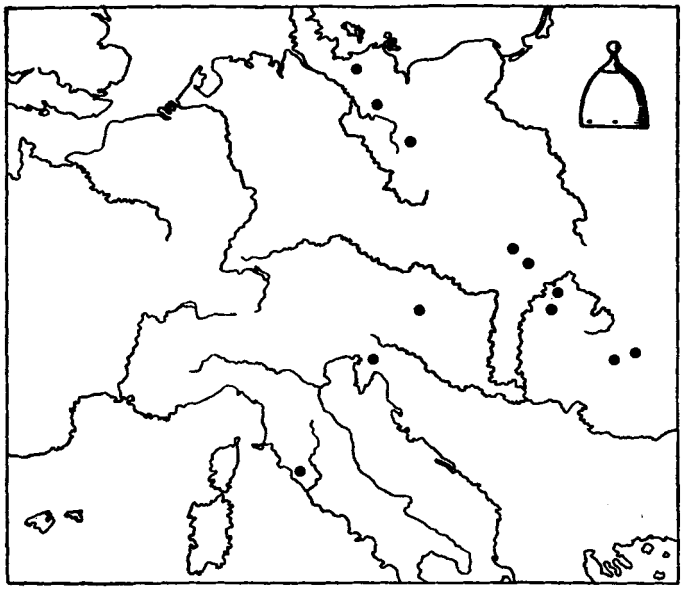

FIG. 2. Distribution of bell helmets (after Merhart, 1941).

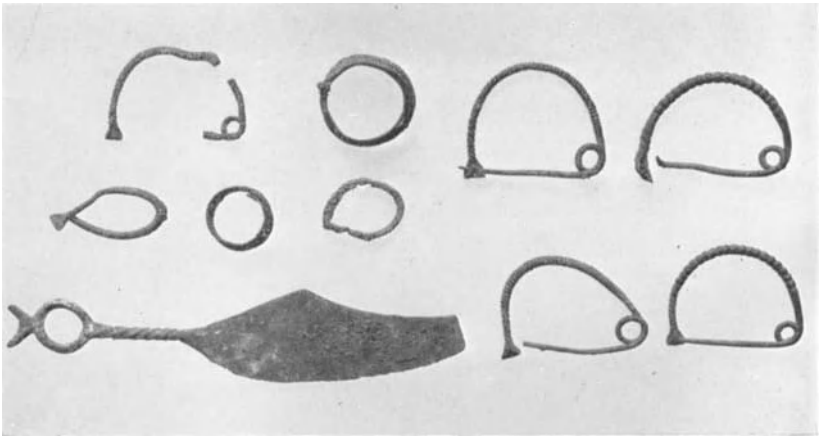

FIG. 3. Razor, fibulae and rings from Fontanella in the Museo Pigorini (photograph Felbermeyer).

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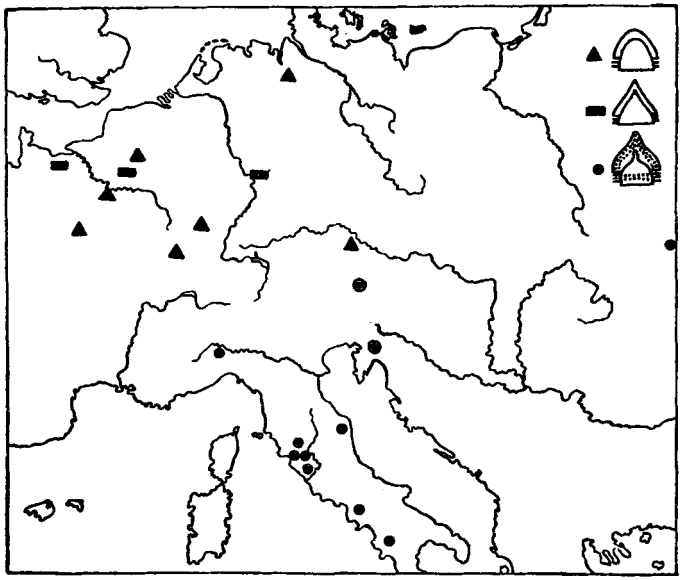

FIG. 4. Distribution of crested helmets (after Merhart, 1941).

examples were included in the map, this type would be much more fully represented in Etruria.). Merhart has also pointed out that the false rivets of the Villanovan helmets are derived from the real rivets of the helmets north of the Alps. Hence, the type must have come from there into Italy.

On the other hand, an element with a northern distribution is

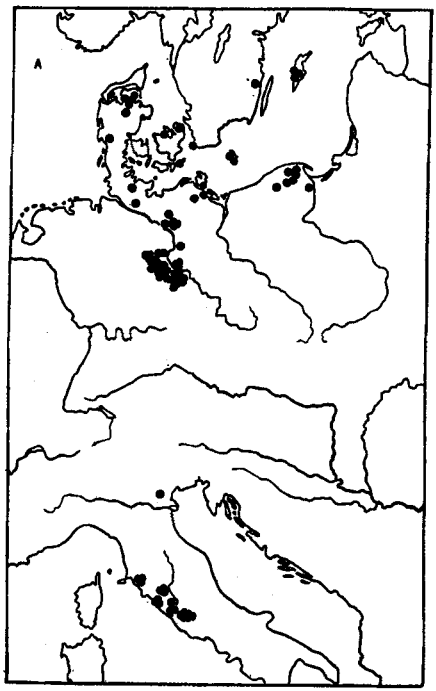

FIG. 5. Distribution of house-urns (after Kossack, 1954).

the house-urn, which is such a striking feature of some Villanovan cemeteries. The distribution of house-urns (Fig. 5) is northern with the chief centre on the Elbe in Germany (Kossack, 1954, p. 60, Plate XXIII A). These belong to Montelius' Bronze Age V which is equivalent to all but the earliest part of

Hallstatt B in central Europe (Sprockhoff, 1950). The Villanovan Iron Age is contemporary with the later part of Hallstatt B. Hence, the German house-urns are of much the same date as the Italian ones.

Another more northern element is the antennae sword, which

FIG. 6. Distribution of antennae swords (after Sprockhoff, 1934).

is a feature of early Etruria and has a more northern distribution (Fig. 6). This type evidently developed in Germany near the middle Elbe, and the earliest beginnings are detectable in Northern Bronze Age IV (Sprockhoff, 1934, Plate XL), a period distinctly earlier than the Villanovan Iron Age.

At the same time, the early Villanovan culture did not lack

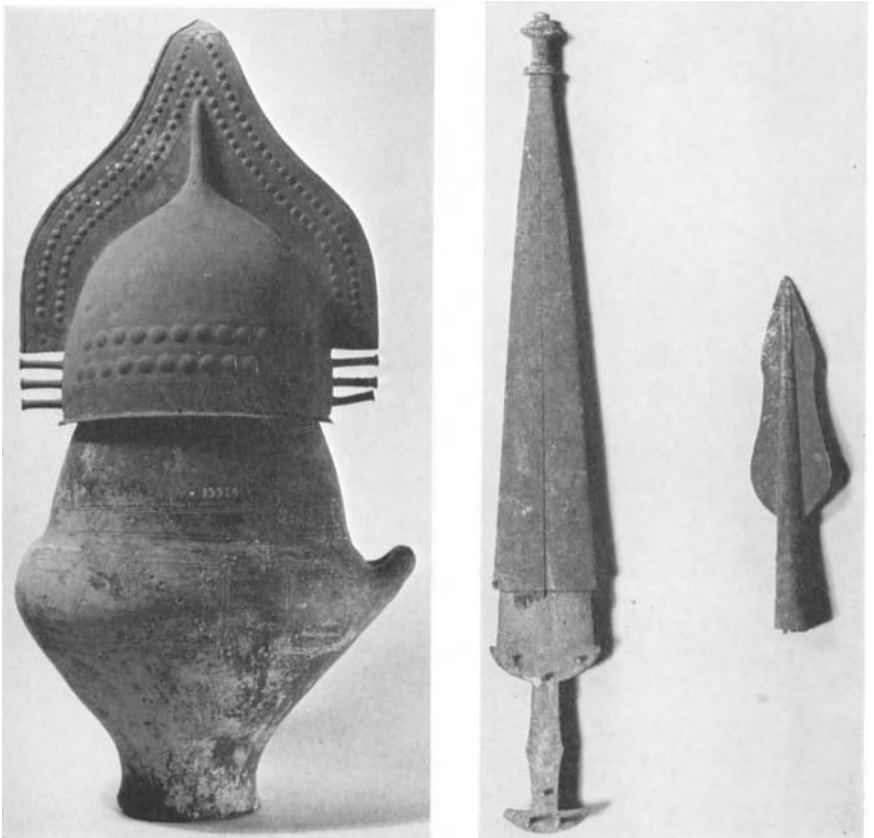

FIG. 6A. Objects from Tomb I, Impiccato, Tarquinia (photograph Felbermeyer).

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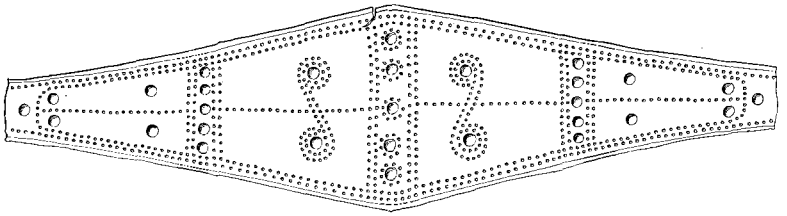

FIG. 7. Bronze girdle from Konjuša, Serbia (after Garašanin, 1954).

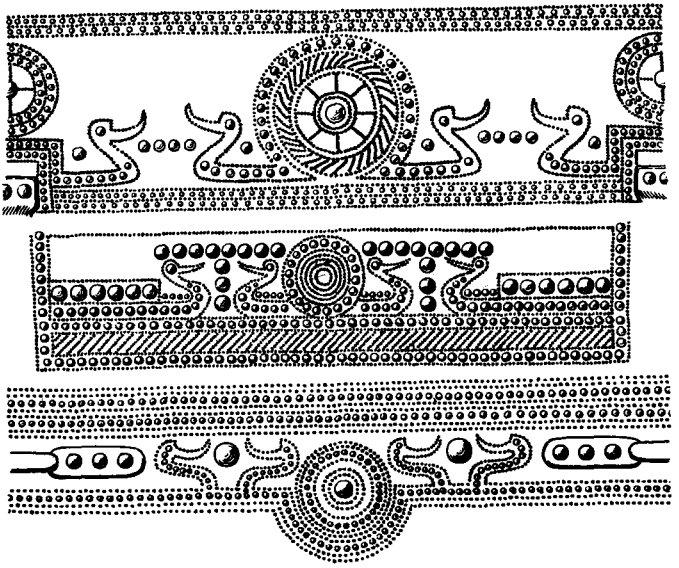

FIG. 8. Ornament from Hungary (after Kossack, 1954).

older Italian elements. I have already mentioned the meanders around the necks of the urns, and these are sometimes filled with dots as on the Apennine pottery. Other elements were quite certainly fibulae and rectangular razors whose forerunners were at home in Italy before the Villanovans appeared. The urnfield peoples were more used to fastening their clothes with pins than with fibulae, but among the Villanovans fibulae are far more numerous than pins. Hence, they adopted an older local fashion in this respect, and one wonders whether this difference did not reflect the adoption of Italian fashions in dress.

Another new element that evidently appears before the end of early Villanovan is red ware, though I cannot suggest an origin for it. It has, in any case, no significant resemblance to the red ware from the Lemnian cemetery of Efestia (Mustilli, 1932–33, Plates VII–X).

But even in the early Villanovan period there appeared T-hilted swords of bronze or iron that recall Greece (Fig. 6A) and little gold bullae that are of eastern Mediterranean origin.

In the second Villanovan period there was a new wave of central European urnfield elements. The bronze girdles were part of this, for the predecessors of the Italian ones are found in central and eastern Europe (Kossack, 1949–50). One of the earliest (Fig. 7) was found in a hoard in Serbia (Garašanin, 1954, Plate XI) with violin bow fibulae of the Peschiera type* and a flange-hilted sword. Garašanin and Kossack have classed this hoard as Hallstatt A or early urnfield and hence considerably older than Villanovan. Also at this time the familiar ornament of birds' heads sometimes flanking circles (Fig. 8) comes in, and which is such a feature of Hungary (Kossack, 1954, Plate VIII). This also occurs on various kinds of vessels of older central European type (Kossack, 1954, Plate XXIII B) and also on Villanovan helmets.

* Säflund points out (1957, p. 12) that such fibulae had a long life in the Balkans, but the bronze sword is here another sign of age. In any case this hoard is not likely to be later than the Italian girdles of the 8th Century.

The centre of production of bronzes ornamented with the birds' heads and circles seems to have been the eastern Alps. These date in large part to Hallstatt B, but some elements such as rows of birds' heads go back to Hallstatt A (Kossack, 1954, p. 46) as does the concept of a circle with twin birds' heads at Grünwald near Munich (Müller-Karpe, 1957, p. 11 and plate VI). These are even older than the first appearance in Italy of such decoration in the hoard from Coste del Marano which is contemporary with earlier Hallstatt B.

Another striking feature is what I call the bull-bird, of which the finest example is the bird chariot of Tarquinia (Ghirardini, 1881, Plate V, 24) (Fig. 9). Small wheeled carts are a feature of the central European urnfields, and they often support a vessel or birds, sometimes accompanied by bulls' heads. In the example from Tarquinia the bull-bird is itself a vessel. The most striking parallel to the Tarquinian chariot is the one from the Glasinac in Bosnia (Seewald, 1939), though it is scarcely older than the one from Tarquinia and may be later (Fig. 10). Another bull-bird, but undated, comes from Hungary, and like the one from Tarquinia it is also a vessel, though it has no wheels (Kossack, 1954, Plates V, XI; Plates IV and V also illustrate related objects.). Several of these bull-birds have been found in Villanovan contexts among which is the splendid example from Bologna generally called a rhyton. The horned bird from the Isis Tomb from Vulci, now at the British Museum, is perhaps a late example. One feels that these objects had a ceremonial purpose, perhaps religious. In any case, this side of Villanovan life is again connected with central Europe.

Beside these new foreign elements, there appears in later Villanovan a fine black polished pottery that looks like a recrudescence of the Apennine Bronze Age and also a possible predecessor of Etruscan bucchero.

The eastern Mediterranean is also represented by more gold bullae and little Egyptian or Egyptianizing figurines and scarabs.

FIG. 9. Bull-bird from Tarquinia in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

FIG. 10. Bird chariot from the Glasinac in Bosnia (after Seewald, 1939).

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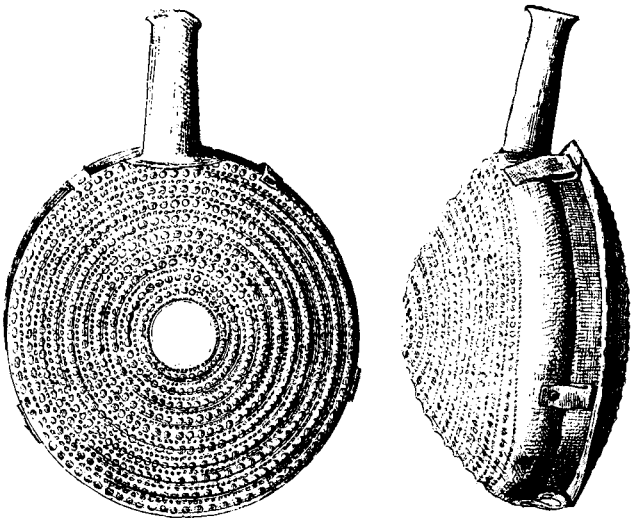

FIG. 11. Bronze flask from the Tomba del Guerriero (after Monumenti Inediti, vol. X).

The bronze "pilgrim flask" * is also another characteristic eastern Mediterranean form (Fig. 11). The T-hilted swords that recall Greece are still in use, and there are imported Late Geometric Greek vases (Åkerstrom, 1943). Also the use of the potters' wheel and of potters' clay is introduced, and the people of Etruria start for the first time to use them to make their own painted pottery in imitation of Greek wares.

Altheim comments in this connexion that traditions of Greeks being concerned in the founding of Etruscan cities are too persistent to be denied. With these may be taken the story of the settlement of Demaratos at Tarquinia (Altheim, 1950, pp. 26–29). Hence, there may well have been a Greek element in early Etruria.

Another factor of uncertain meaning is the change in the mode of burial at Tarquinia, for inhumation now begins to supplant cremation. This was, of course, the normal Italian rite before the urnfields came in, and at Caere it was used side by side with cremation during the Villanovan Iron Age (Vighi, 1953). But now rough stone coffins are sometimes employed, much too rough to be called sarcophagi, but shaped to hold an unburned and extended corpse. True, in earlier Villanovan times rectangular stone coffins of similar roughness had been used for cremations in house-urns. But did this use of inhumation in coffins represent influence of Greeks or possibly eastern Mediterranean settlers, or did it reflect a mixture with the people of the Fossa Grave Culture to the south, as in contemporary Rome (Säflund, 1957, p. 10*f*)? Another element in burial custom at this time is the appearance of a very few chamber tombs (Minto, 1921; Helbig, 1888). Rock-cut ones might point no further away than Calabria or Sicily, but it is hard to separate them from the somewhat later masonry chamber tombs covered by tumuli which do suggest western Asia.

* For example, the one from the Warrior's Tomb at Tarquinia (Montelius, 1895, Plate 289, 6).

Now to sum up so far, I believe that the Villanovans were an urnfield people of central European origin with an especially strong eastern European element* but, like other early colonists in the Mediterranean, were a mixed group and consisted of urnfield elements from far and wide. Also, like other early Mediterranean settlers, they mingled with the previous population, though this mixture was in their case less apparent at the beginning of the settlement than later on when they had had time to mix with the older inhabitants. Their settlement was a development parallel to that of the Fossa Grave people further south whom Säflund would bring from the Balkan–North Aegean region (Säflund, 1957, p. 14).

A purely speculative question is the language of the Villanovans. First of all one can say that, in the area covered by the urnfields, the languages at the dawn of history were Indo-European like Celtic and Illyrian or had a strong Indo-European element like Ligurian. To be more specific, it must have been the urnfield people who introduced Celtic into Spain (Hencken, 1955, p. 14), and it is hard to doubt that the urnfield people of Este were the bringers of Venetic to north-eastern Italy. If this reasoning holds good, the Villanovans would also have spoken an Indo-European language (see also Riis, 1953; Hencken, 1955, p. 24) and hence would not be responsible for the non-Indo-European element in Etruscan. This is not to say, of course, that Indo-European speech was brought into Italy *only* by urnfield settlers. I should warmly agree with Pallottino that the inhuming Apennine Bronze Age and perhaps the even earlier Eneolithic culture would have come from the Balkans, probably bringing Indo-European (Pallottino, 1957*a*, pp. 36–37, 43, 51–52. For the traditions of a settlement from the east, see Pallottino, 1955*a*, p. 3). Zambotti (1954, pp. 444, 450) believes that both the Bronze Age people and the Villanovans came from the same direction, and

* See also von Vacano (1955, p. 52). The relationship of the urnfields and northern Europe to Villanovan has also been studied by Kossack (1950).

that both were Indo-European speaking. Probably one should also include the Fossa Grave people. It seems to me that this is likely to be right, and it also seems to me improbable that either of them spoke an old Mediterranean non-Indo-European language (Pallottino, 1955*b*, p. 28).

We now come to the third period in Etruria embracing most of the 7th Century. Since in this period we have the first Etruscan inscriptions, we do not need to ask any longer who the people of Etruria were or what language they spoke. I shall not attempt any description of the 7th Century in Etruria, for this is far too wide a subject, but it was marked by the appearance of a much more highly developed culture (Bloch, 1954, p. 6). I shall limit myself to a brief discussion of two tombs that seem to me significant in the question of who the Etruscans of this time were.

One is the Bocchoris Tomb at Tarquinia. I do not suppose that anyone would hesitate to call this tomb Etruscan, and I propose therefore to analyse its contents, relying on Helbig's description (Helbig, 1896)* and on the objects that can now be identified by it. Here we find ourselves in a new world. First there were Oriental elements. There was not only the Bocchoris vase itself but fragments of other faience vases. There was also a necklace that originally consisted of no less than 91 little faience figurines (Fig. 12). It is not necessary to argue here whether these are really Egyptian or merely Egyptianizing, since they are obviously one or the other. (Drs. Dows Dunham and William Stevenson Smith of the Boston Museum of Fine Arts consider that these objects were made in Egypt.) There are also two large pottery stands (Fig. 13) imitating bronze ones of a type that Mrs. Maxwell-Hyslop (1956) has traced to Urartu.

Pallottino (1957*b*, p. 92) has pointed out the chronological difficulties in Mrs. Maxwell-Hyslop's view, but there is no doubt that the shape of the stands is Oriental. Hawkes and Smith

* Montelius (1895, Plate 295). Not all objects figured by Montelius are in Helbig's account, and Helbig describes others that Montelius did not figure.

(1957) have suggested ways of bridging these difficulties. A gold object (Fig. 14) showed a woman between two sphinxes that look Assyrian (Montelius, 1895, Plate 295, 3). Two handles for a large bronze vase were topped with Oriental lotus flowers.

There was also a Protocorinthian element in this tomb represented by the broken oenochoe and the kotyle (Fig. 15), but these are of course imitations and not imports, and the curious bird-headed animals on the kotyle contain a local element. My colleague, Professor Hanfmann, informs me that he would place the kotyle in the first quarter of the 7th Century.

But there were also Villanovan elements. Helbig describes three cups, which cannot now be identified, that he says were descended from those in the Villanovan graves. Another larger cup is of a new shape, but the ware is intermediate between the the finer Villanovan pottery and bucchero (Fig. 16). It is also decorated with very faint white paint like the Villanovan house-urns. Other Villanovan elements are the false cord ornament and the stamped designs. A large fluted jar of red ware (Fig. 17) is the descendant of a type whose primitive beginnings are in early Villanovan and whose more developed representatives appear in later Villanovan.

Perhaps the most remarkable vase from the Bocchoris tomb contains all three features, Greek, Oriental and Villanovan (Fig. 18). Around it is a frieze of warriors which, though rendered in the old Villanovan white paint, is derived from Greek prototypes. Their jutting noses and beards, the enormous eyes, their prominent lips and the helmets with raised crests all recall the earlier 7th Century in Greece. But Protocorinthian warriors are often shown in lively action. For the quiet pose of these warriors one may turn to early Protoattic ones shown on a ship (Fig. 19) (J. M. Cook, 1934–35; see Plates XL*b*, XLVII–L, p. 173, Fig. 2). These Cook would date between 700 and 690 B.C. The Aristonothos crater (Fig. 20) which, according to R. M. Cook, belongs to the second quarter of the 17th Century, has

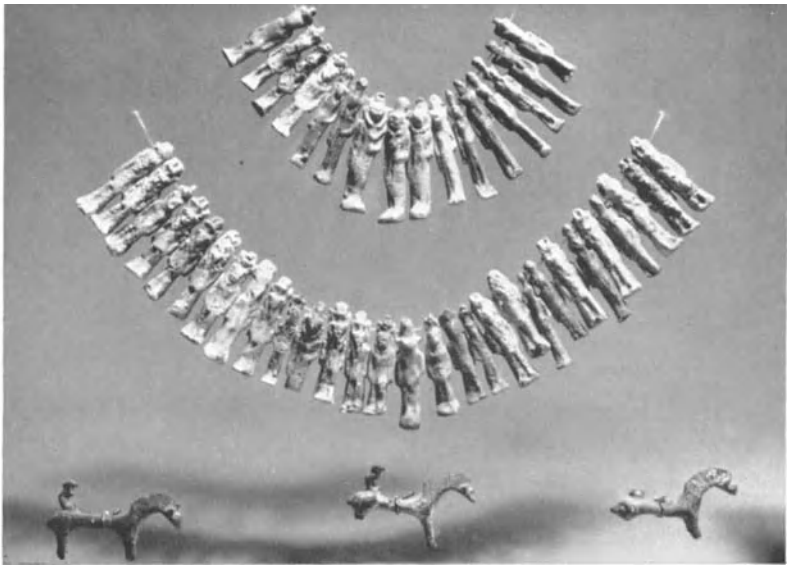

FIG. 12. Faience figurines and fibulae from the Bocchoris Tomb in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

FIG. 13. Pottery stand from the Bocchoris Tomb in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

facing page 42

FIG. 14. Gold plaque, Bocchoris Tomb (after Montelius, 1895).

FIG. 15. Vases from the Bocchoris Tomb in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

FIG. 16. Cup from the Bocchoris Tomb in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

FIG. 17. Red jar from the Bocchoris Tomb in the Museo Nazionale Tarquiniense (photograph Felbermeyer).

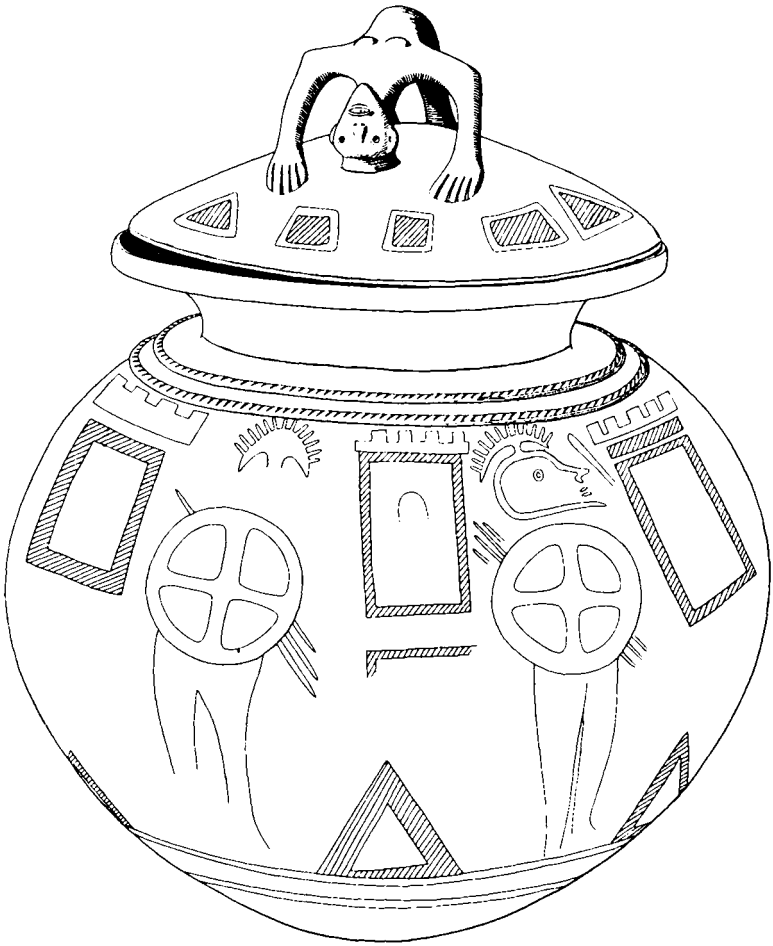

FIG. 18. Jar from the Bocchoris Tomb in the Museo Nazionale Tarquiniense.

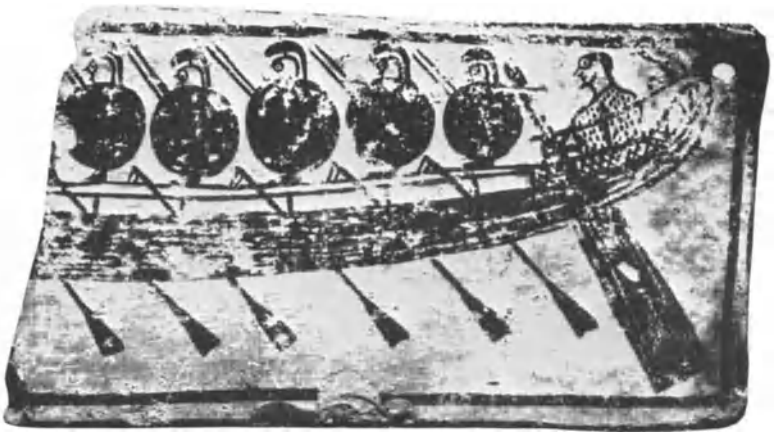

FIG. 19. Protoattic plaque (after Cook, 1934–35).

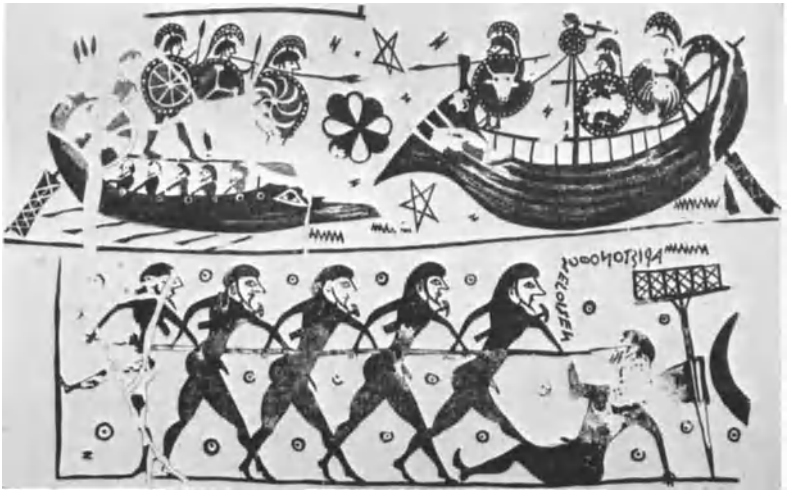

FIG. 20. Aristonothos crater (after Pfuhl, 1923).

FIG. 21. Urn and pottery helmet (see opposite page) from Tomb 66 at Tarquinia. Museo Nazionale Tarquiniense and Museo di Villa Giulia, Rome (photograph Felbermeyer).

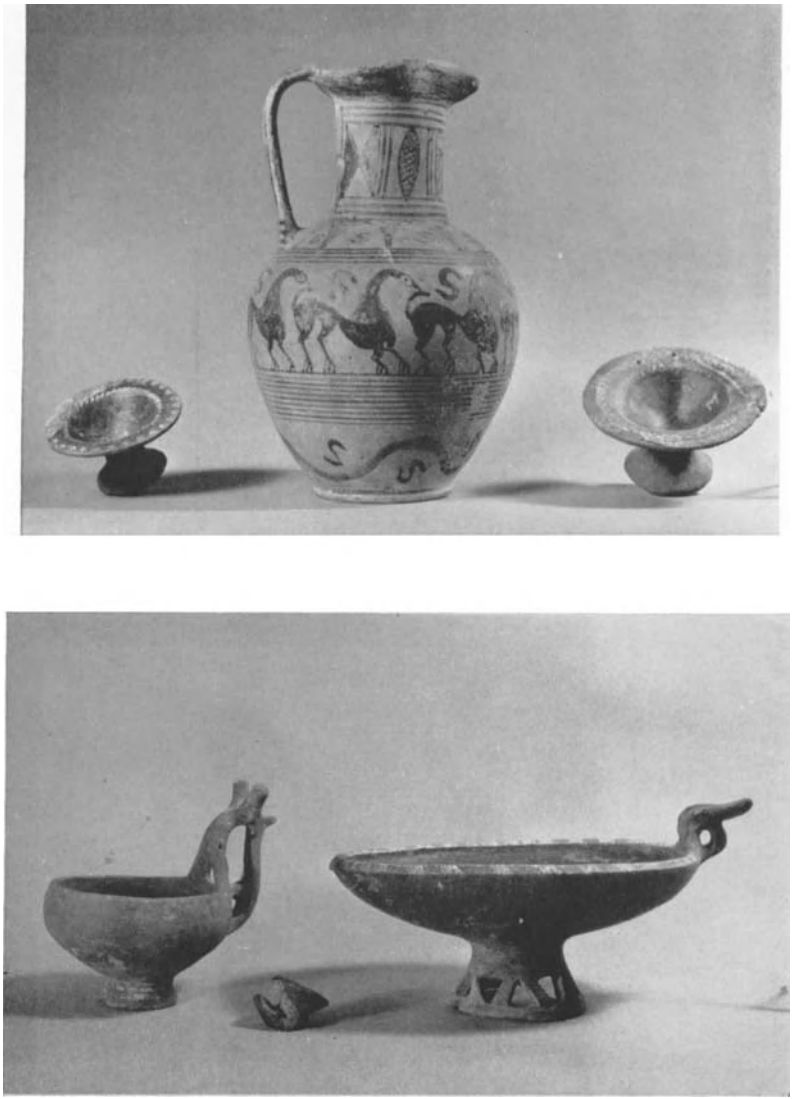

FIG. 22. Vases from Tomb 66, Tarquinia, in the Museo Nazionale Tarquiniense and Museo di Villa Giulia, Rome (photograph Felbermeyer).

similar rather stiffly posed warriors on a ship and also men with jutting beards and noses and prominent eyes and lips (Pfuhl, 1923, Plate XIV, Fig. 65). Hence, this does not gravely conflict with a date in the first quarter of the 7th Century for the kotyle from this grave.

In any case, the warriors on this vase from the Bocchoris Tomb have no special resemblance to such Asiatic warrior figures as those from Carchemish, Kara-Tepe or Sinjerli.

There are also two Oriental elements in this vase. One is the figure on the lid that Hanfmann (1936) considers to be an Egyptian dancer. The other is the series of towers between the warriors which resemble the towers on Cypriot bowls.

But Villanovan elements are also present. First the jar itself is made of the old red ware that seems to have begun even in early Villanovan. The second is the white paint with which the warriors are painted.

Hence we see in this one vase, as in the Bocchoris Tomb in general, a very strange mixture of Greek, Oriental and Villanovan elements.

The last grave that I shall mention is Tomb 66, also from the Monterozzi cemetery at Tarquinia. This was a Villanovan pozzo grave and was published by Romanelli (1943, Figs. 1, 2, 3). It contained an ordinary Villanovan urn but covered by a pottery helmet of Villanovan type which is much more elaborately ornamented than any other of its class (Fig. 21). Part of the decoration is again in faint white Villanovan paint. Other pots from the grave are of Villanovan types but unusually advanced in character (Fig. 22).

The most remarkable object from Tomb 66 is an oenochoe of Protocorinthian shape (Fig. 22) but with exactly the same animals with birds' heads as on the kotyle from the Bocchoris Tomb (Fig. 15). Hence, these two tombs must be near contemporaries in the first quarter of the 7th Century.

Clearly the Bocchoris Tomb shows a very different world

from that of the Villanovans. The Oriental elements are very prominent, but they do not seem to reflect any one area of the eastern Mediterranean. Pallottino (1957*a*, pp. 72–73, 77) points to elements at this time from Egypt, Mesopotamia, Syria and the Aegean world which he suggests were the result of Phoenician and Greek trade. No one would rule out Greek and Phoenician trade as one factor in this complex situation, but it seems to me that these elements could also reflect a colonization like the prehistoric ones in the Mediterranean that consisted of various mixed elements and which itself intermingled with the older population. Some foreign types could of course be the result of trade. But such striking things as chamber tombs filled with rich objects of foreign type suggest more than mere commerce. Rock-art chamber tombs by themselves need come from no further away than Calabria or Sicily, but 7th-Century ones with architectural chambers and large tumuli like the Regolini-Galassi tomb at Cerveteri or the Tarquinian Tumulo del Re at the Doganaccia (Cultrera, 1932) may represent not mere trading contacts but a new, wealthy and dominant element from much further afield (see also Riis, 1953).

I would also point to the Villanovan elements in the Bocchoris Tomb and to the markedly late Villanovan character of Tomb 66 as indicating that, at the beginning of the 7th Century, Villanovan elements still existed by themselves and also existed partly combined with Oriental and Greek elements. I would suggest again that this conforms to the prehistoric Mediterranean pattern of new settlers mixing with the older inhabitants.

In other words, I would agree with Pallottino's concept of ethnic formation, but in a somewhat different sense. He asks who are the French—the Gauls, the Romans or the Franks? I would parallel this by asking who are the Etruscans—the Eneolithic people, the people of the Apennine Bronze Age, the Villanovans, the Greeks or the bringers of Oriental elements? My answer would be that the Etruscans of history were a composite of all

these elements (see also Riis, 1953; von Vacano, 1955, p. 62; Bloch 1954, p. 13; Altheim, 1950, p. 34), and of course, as Pallottino (1957*a*, p. 89) has observed, this process could only have taken place in Etruria. I should also emphasize that until the Etruscans can be clearly identified by name and language in the Near East, I would rather reserve the name Etruscan for the mixed people of the 7th Century and later without trying to decide to which element it should belong. There remains the problem of the language and its origin. It seems to me that the archaeological evidence suggests that the Villanovans, the Apennine Bronze Age people and perhaps even the Eneolithic people of Etruria were Indo-European speakers. If one can accept this suggestion, then the Etruscan language must be either pre-Bronze Age or it must have come in with the Oriental elements that became dominant in the 7th Century.

Finally, it seems to me that we should give up trying to decide whether Herodotus or Dionysius was right in his views on the origins of the historical Etruscans. If Herodotus said that they came from the eastern Mediterranean and Dionysius said that they were indigenous, each was telling part of the truth.

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GROUP DISCUSSION

Ward Perkins: Dr. Hencken has put the social and cultural structure of the Villanovans in a new perspective. He has pointed out that at Tarquinia the whole area of the later Etruscan city was occupied already in the Villanovan period probably as much as it ever was in the Etruscan period. That is a phenomenon which is becoming increasingly obvious at a number of other later Etruscan sites. For instance, it is quite clear that at Veii the whole area of the city, though not densely occupied, existed as a city at Villanovan times, with Villanovan cemeteries at the principal gates. Therefore, one can no longer speak of a sudden change from primitive Villanovans to highly civilized Etruscans. A great deal of what we find in historical Etruria by way of social organization and culture was certainly already present among the Villanovans. This point has emerged very clearly from the recent work in Etruria.

A point which arises from the surveys which we have had from Drs. Foti and Ciotti is that one unfortunate little people got missed out, simply because they were neither Etruscans nor Umbrians—namely the Faliscans, who are none the less extremely relevant to our studies. This people centred on the site of Falerii, the modern Civita Castellana, on the right bank of the Tiber north of Veii and south of the territory of Volsinii. They are of particular interest to us because, although their remains are culturally indistinguishable from those of the Etruscans, we know from their inscriptions and from classical sources that they continued to speak an Indo-European language.

We have a mélange of inscriptions from their territory, some in Etruscan and some in a dialect closely related to Latin. Theirs is a marginal territory where one can hope to find evidence for unscrambling the jigsaw of Etruscan and non-Etruscan elements, particularly in the sphere of language. In this connexion, it is clear from Mr. Cook's introductory address that in all of this discussion we are up against serious problems of terminology. As Dr. Hencken pointed out, it is probably only justifiable to talk of the Etruscans, as such, when we are

dealing with the historical people whom we know of as the Etruscans in the 7th Century B.C. Prior to that, one may carry back elements of their culture, their language and genetical elements to an earlier phase; but to equate any of them, on present evidence, at an earlier stage in our terminology would be to invite disaster.

Mr. Cook has already indicated an analogy with the Norman invasion of England. A possible analogy is to be found also in Italian studies, namely that of the Normans in Sicily and southern Italy. There, one had a small dominant minority, who were politically and organizationally in complete charge of southern Italy for a couple of centuries but who, archaeologically speaking, are almost entirely mute. A very interesting problem would be to study, from the genetical point of view, the graves of 12th-Century Sicily. The analogy is worth bearing in mind. It may not be what happened in Etruscan Italy; but it could very well have happened. The problems of culture, language, genetics and political entities are four separate problems which may coincide at certain points, but which may just as easily diverge.

de Beer: Mr. Cook, in his admirable introduction and his analysis of the disciplines which can be brought to bear on this problem, mentioned philology, archaeology and genetics, but physical anthropology should not be forgotten, unless it was included under the term of genetics; because the shape of your skull is also determined by genes which are inherited from your ancestors. It is important to bear that in mind, particularly in view of the very careful studies which Giuseppe Sergi made on highly critical material, and which I hope will be continued on fresh material as and when it may become available.

Medical Biology and Etruscan Origins
G. E. W. Wolstenholme,Cecilia M. O'Conner
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FROM THE VILLANOVAN CIVILIZATION TO THAT OF THE ETRUSCANS

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A CENTURY ago, rich necropolises dating from the first Iron Age were discovered near Villanova, in the neighbourhood of Bologna. Count Cozzadini carried out a methodical exploration of these sites. Since that time the cultural *facies*, first revealed by these discoveries in the Po valley, assumed an ever-increasing importance for research. It was later found, with the same characteristics, in various other parts of the Po valley, but also at many important sites in Tuscany, where powerful Etruscan towns were to develop. In many cases it appears that the Etruscan civilization directly succeeded the Villanovan one. In Latium, a cultural form extremely similar to the Villanovan *facies* occurs in the Alban mountains and in Rome itself. Furthermore, the same archaeological *facies* has been found in the Marche, with the recent discovery of the necropolis of Fermo (Annibaldi, 1956), and in Sicily, at Milazzo, where Bernabò Brea uncovered a stratum of typical Protovillanovan character (Bernabò Brea and Cavalier, 1956).

Thus, the extent and importance of this civilization of the first Iron Age may be seen, while contemporaneous civilizations with very different characteristics are seen to have developed in various regions of eastern Italy, in Umbria, Piceno, Abruzzi and Puglia, and of southern Italy, from Campania to Calabria. The most notable characteristics of the Villanovan civilization are cremation of the dead and deposition of their ashes in ossuaries of biconical form, and the geometrical decoration of pottery, bronze and iron

objects. Towards the middle of the 8th Century B.C., there occurred a modification: inhumation of the dead, with an increase in richness of the offerings. Thus, we can distinguish a primary and an evolved phase of this civilization.

It is remarkable that, in time and space, the Etruscan civilization immediately succeeded the Villanovan, at the beginning of the 7th Century in Etruria and at the end of the 6th Century in the Po valley. The difference in time is understandable in itself: the Etruscan civilization began in Tuscany around 700 B.C., and the conquest of the trans-Apennine plain by the Etruscans did not take place before the end of the next century. This last case presents no problem. The Italic people near Bologna and in the Emilia were defeated by an army which was superior in technique and in quality, the army of an already powerful population. But how are we to explain what had happened two centuries before, in Tuscany, i.e. the succession of the Orientalizing to the late Villanovan period? There we are at the very heart of the problem of the origin of the Etruscans. Are we to suppose that a new population arrived, bringing a higher civilization; or that there occurred a progressive transformation of a culture, without ethnic modification? Although I shall not discuss the central theme of our symposium, I shall attempt to clarify some points that I think important.

If a change took place in the Tuscan culture around 700 B.C. it was not marked by an abrupt break with the recent past, as was clearly shown by Pallottino (1947). The sites of the Villanovan and Etruscan necropolises are very near each other; although the Villanovan centres have often disappeared, we must conclude that these centres and the Etruscan towns did not occupy very different sites. The form of the pottery or bronze objects remains the same or is slowly modified. Thus, the Etruscan civilization, although new, comes from a preceding culture where we find many of its roots. From the ethnic point of view, the sequence is clear. It would not be true to say that among the

greater part of the population—let us call them Italic—the Iron Age civilization disappeared, in the 8th Century B.C., at the time of the appearance of the Etruscan civilization; and we must eliminate, in any case, the suggestion of a general or profound ethnic change. If Oriental elements arrived on the Tyrrhenian coast of Italy, they were numerically few and did not change the essential substratum of the preceding *ethnos*.

The progressive character of the cultural transformation from Villanovan to Etruscan is more evident in towns at a distance from the sea. This may be illustrated by a discovery made during Franco-Italian excavations, directed by the present author during the last ten years near Bolsena. In this part of the interior of Etruria, more than 100 km. from the sea, we found the successive appearance of three successive cultures, Apenninic, Villanovan and Etruscan, a succession very rarely seen in Etruria (Bloch, 1955*a*). On an isolated hill, *La Capriola*, we discovered a series of traces of dwellings hewn out of rock and forming a fairly large village. These dwellings are dated by the pottery found there, which is very simple, hand-made, and shows the characteristics and drawings typical of Apenninic pottery. Thus, it is a centre dating from the end of the Bronze Age. The civilization so characterized is known as Apenninic. It is found largely along the mountain chain of Italy, but until now had almost never been found in Etruria. One of the very rare and well known Apenninic centres excavated in Etruria is that of Belverde, near Chiusi. We have not yet found the tombs belonging to the centre of La Capriola. Instead, on the lower slopes of the hill we found a late Villanovan cemetery, with rich and varied material.

So the second link of the chain appeared. The Villanovan culture was represented by thirty tombs, in the form either of trenches or wells found at a very short distance—between 50 and 100 cm.—from the surface of the ground. The offerings formed with the earth which filled the trenches and wells compact and very hard masses; they date from the beginning of the

7th Century B.C., and consist of pottery, bronze and iron objects (Figs. 1, 2). The vases vary in shape and are sometimes very elegant. The decoration is simple and geometrical as always in objects produced in the pre-Etruscan period; and is carried out in graphite or paint. Many of the big vases are *olle* which contained the ashes of the dead. A number of the graves discovered were those of soldiers, and many fragments of weapons have been found: iron heads of javelins and of lances, short heads of arrows, and knives (Fig. 3). All these heads were primitively fixed on wooden handles. No intact handles have been found, but some fragments of wood were still attached to these metal heads.

Near this armature of iron weapons, which constituted the power of the people who had it, we also found some made of bronze. The appearance of iron in Italy at the beginning of the first millennium B.C. did not lead to the complete disappearance of bronze, which had been in use for centuries. On La Capriola we found many bronze buckles in the form of leeches (Fig. 4); the curved bow is ornamented with graphite; the geometrical decoration consists of straight or intersecting lines, drawn with skill. Some of these buckles have the exceptional length of 12 cm. Many other bronze objects constituted the element of richness of these primitive tombs: bronze bowls which have been almost completely destroyed with time; sword-hilts; a very well preserved breast-plate (Fig. 5); combs and crescent-shaped razors.

The most interesting piece is a small bronze shield, 33 cm. long and 30 cm. wide. It consists of a thin plate of bronze which has two small symmetrical cavities, one on each of the smaller sides (Fig. 6). Two series of large bronze nails, on the inside, were used to attach the two bronze ribbons through which passed the leather strap for carrying the shield. This shield, exceptional in form, was too small to be used as a weapon and must have been a votive object, or perhaps it was for ceremonial use in display or in the dance. The two small cavities allowed freedom and rapidity of

movement to the arm. Despite the difference in size, this weapon recalls the sacred shields of the *Salii*, warrior priests of Rome, who danced while beating with a lance an oval shield which had two very big cavities, one on each of the longer sides.

Finally, about 100 m. from La Capriola, on a hill with the suggestive name of *La Cività*, we found an archaic Etruscan centre quite undamaged by the Romans (Bloch, 1955*b*). A primitive wall was uncovered at some points along the perimeter of the hill. In the centre were the ruins of a sacred edifice, abandoned at some far-distant time, probably at the end of the 5th Century B.C. The floor of the temple was covered with fragments of impasto and of black and grey bucchero which could not belong to a period later than the 5th Century B.C. The temple comprises only one large room with a *pronaos*. Like the rock temple previously found in the immediate vicinity of Bolsena, this building was wider than long (12 m. wide and 11 m. long). It was, of course, not built of limestone; the walls were constructed with perpendicular blocks of tufa and heaps of stones. This technique is rightly described by Lugli (1957) as *nervature litiche*.

The interior of the room is similar to that in the rock temple near Bolsena. Two parallel blocks of tufa probably formed the base of the pilasters which, together with the walls, supported the weight of the roof; a quantity of the roof tiles have been found. In the middle of the room, a round slab of stone supported perhaps the cult image. We found many fragments of what were parts of a ritual offering to the divinity.

The zone around La Cività is also of great archaeological interest. Many trench or chamber tombs of the 5th and 6th Centuries B.C. were found very near the Villanovan necropolis. Generally these tombs contained a slight amount of impasto and bucchero vases with few bronzes. Many of them were robbed by unknown diggers. In a trench tomb, dated probably around 600 B.C., was found a funerary altar of a type which is extremely rare in Etruria. Three rows of holes were hollowed out of the

FIGS. 1 and 2. Pottery and bronze objects from a tomb at La Capriola.

facing page 54

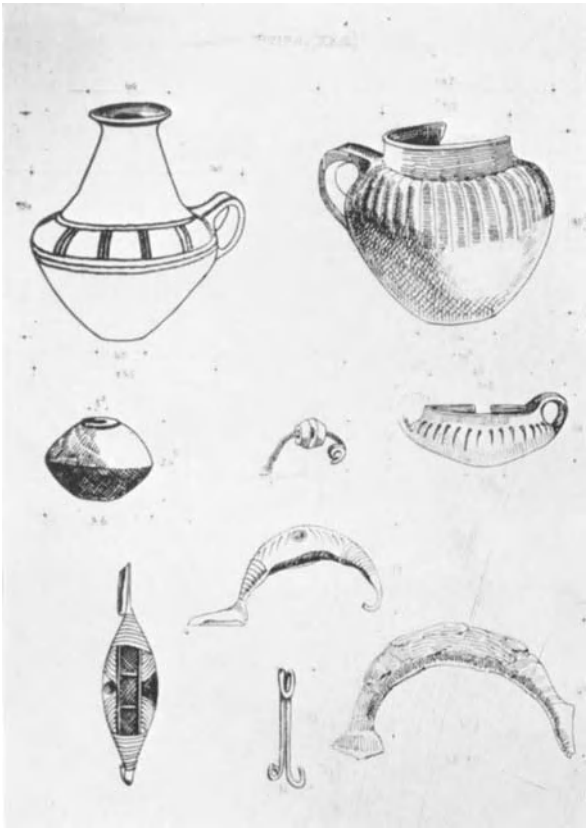

Fig. 2.

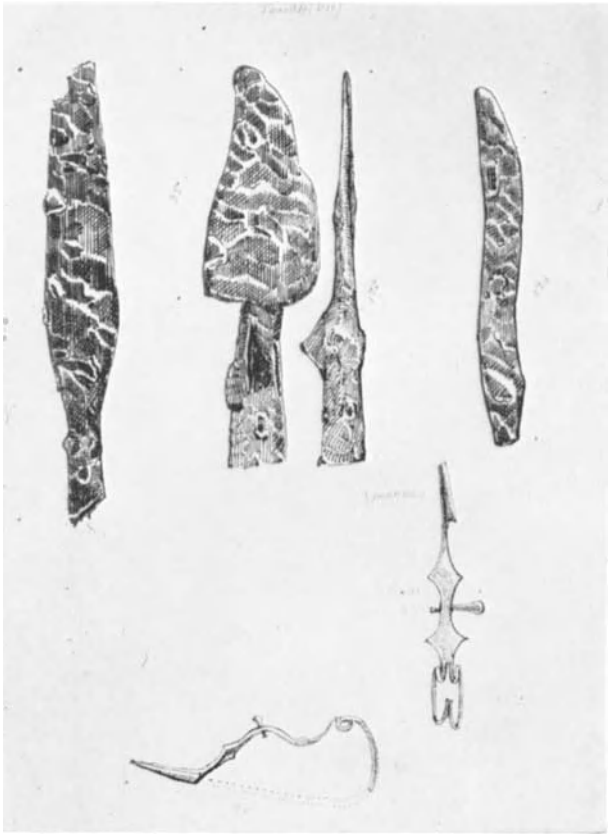

FIG. 3. Fragments of iron weapons and a bronze fibula from La Capriola.

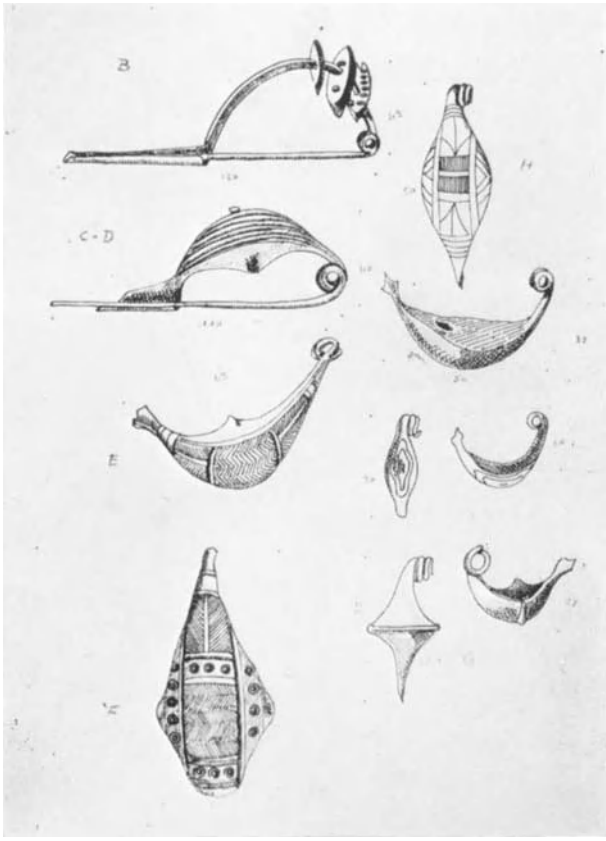

FIG. 4. Bronze fibulae from La Capriola; one is set with amber.

FIG. 5. Bronze breast-plate from La Capriola.

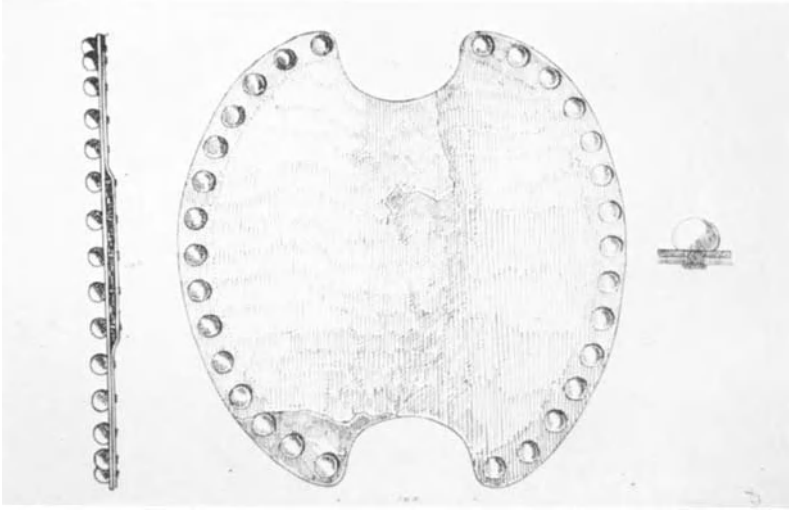

FIG. 6. Bronze shield from La Capriola.

surface, and in these were placed the first corn and some drops of the new wine, offerings particularly welcome to the dead.

Thus, in this isolated part of inner Etruria, inhabitants of the Bronze, Iron and Etruscan Ages succeeded one another on nearby hills. This continuity of successive civilizations is a remarkable fact. The occurrence of an Apenninic civilization is most exceptional in Etruria. Then the transition from Villanovan to Etruscan is very gradual here. The earlier Etruscan phase is characterized by the total absence so far of Oriental objects, by the spread of bucchero and by the construction of typical buildings. The only point in common with the Orient is that of the altar with holes, a true funerary altar, which finds its model in many Oriental civilizations, e.g. Punic, Judaic and Hittite. Everything suggests that, in this isolated region of Etruria, the Etruscan civilization acquired a rather provincial aspect and advanced only slowly. The form of the bucchero and impasto vases of the Etruscans evidently derives from that of the Villanovans. Thus, perhaps more than anywhere else, the continuity of the cultures is clearly shown here and, without any doubt, the continuity of the populations.

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ORIENTAL CHARACTERISTICS OF THE ETRUSCAN RELIGION

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AMONG the institutions of any people, religion is the one which is most closely bound to their traditions, does not readily admit modification and often survives uncorrupted throughout centuries.

For this reason, if we could prove the existence of a close link between the Etruscan religion and those of the Orient, we could conclude that the Etruscan civilization was elaborated in contact with Asian populations. The case of the affinities between the religious thought of the Etruscans and the Babylonians, for instance, is quite different from the case of similarity in equipment, garments or jewellery. Equipment and objects of art were brought by merchants, but the influence of the religious thought of foreigners could never be more than superficial and fleeting.

The subject of this paper is one that has been extensively studied. Clemen (1936) said, with great prudence, that it is possible but not certain that the Etruscan religion was influenced by the Orient but, in the same year, de Ruyt concluded from his study of the monsters in Etruscan pictures that these confirmed the Oriental origin of the Etruscans.

It is now possible to assemble a number of known facts which prove the existence of close affinities between Etruria and the Orient. Firstly, there is the fact that, according to Varro, the supreme God of the Etruscans was the sky; they called their principal god Οὐρανος, writes Lydus. In the words of Nilsson (1950), discussing the history of the religion of the Greeks, whose

chief god was the sun: "There is also another theology which places above all the universe the sky god, called by the Semites Baalschamin, by the Greeks Zeus Uranios and by the Romans Coelus or Jupiter *summus exsuperantissimus*. This theology is certainly Oriental, as it is related to . . . Babylonian cosmology . . . and astrology."

According to Seneca, Jupiter represented *animum et spiritum mundi*. Now this is the exact definition of the sky cult, found in Plato's *Timaios*, a dialogue written in the last years of the master under the influence of the Orient.

The supreme god of the Etruscans is the sky, and the other gods have an astral character. Thus, the Etruscan religion cannot be compared with the naturalistic polytheism of the other populations of Italy. The best available description of the Etruscan cosmology is found in a very late text, by Martianus Capella (see Weinstock, 1946). Here we learn that the supreme god, Jupiter, sends his *milites* to call his advisers from sixteen points of the sky. Twelve are even colleagues of Jupiter. The text mentions *septem residui* and also *dei azoni*. It would be absurd to doubt the reliability of Martianus Capella, whose information, according to Wissowa, came from a very good Etruscan source through the work of Nigidius Figulus. The catalogue of the astral gods, found in the writings of Martianus Capella, is almost the same as that which Diodorus describes as the Chaldean one. In the catalogue of Diodorus are found the council of the gods, the twelve *principes deorum*, the stars which are placed outside the zodiac. There are some discrepancies between the catalogue of Diodorus and that of Martianus Capella, e.g. Diodorus has only five planets, whereas Martianus Capella has seven, and, in addition, the sun and the moon. But the greatest difficulty is this: are the twelve gods those of the zodiac? If so, the actual form of these cosmogonies could not date from before the 5th Century B.C. Meanwhile, the cosmogonies of the Etruscans and of the so-called Chaldeans are almost identical, and are composed of

elements which, according to Weinstock (1946), are neither Greek nor Roman.

Philosophers, when they read in Plato's *Epinomis* that the true gods are the stars, suspect an Oriental influence, transmitted perhaps through the mysterious Chaldean who was among Plato's last pupils.

Before leaving the question of the gods, mention should be made of some resemblances that have been suggested: (1) The name of the goddess *Turan* derives from the Asiatic word *tur*, meaning almighty, according to Przluski (1932). (2) The name of the hero *Tarchon* originated in the region of Mount Taurus. (3) The *Culšanš* and the *Culšu* of the Etruscans are divinities similar to the Kulšeš of the Hittites, according to Hrozny (1929); but this relationship is not accepted by Devoto. (4) The name of the Etruscan Fury, *Lasa*, seems similar to the name of the infernal divinity of the Babylonians, *Las* or *Laz*, but this relationship is not accepted by Clemen.

It is also very surprising to find in the Etruscan tradition an account of the creation of the world similar to that of the Bible. Such a tradition, in fact, is found only in a text by the mediaeval writer Suidas, Τυρρηνία, who refers to a source that he imprecisely calls ἔμπειρος ἀνήρ. Pallottino believes that the version of Suidas may be explained as a corruption of Etruscan with Christian elements in late Roman times. Pallottino's hypothesis, however, is contradicted by the fact that in a pehlevi text of the Sasanian period we again find the tradition, unknown in the Bible, which limits the span of the universe to 12,000 years. Therefore, the Etruscan and pehlevi texts must derive from a common Oriental source.

The Etruscans were skilled in divination by inspection of the livers of victims—this was the field of the so-called haruspices—or by observation of lightning.

The name "haruspices" was related to the Chaldean word *har*, meaning liver, by Boissier (1900), but this observation was ques-

tioned by Iastrow (1907). Scholars have long been interested in the bronze liver found at Piacenza, which without doubt was used for the instruction of the priests. It is well known that hepatoscopy originated in Babylon. Livers similar to the one found at Piacenza were found at Boghaz-Köy and Mari. However, the Babylonian origin of the Etruscan practice of hepatoscopy was denied by Furlani, who speaks, instead, of polygenesis. A liver, dating perhaps from the 2nd Century B.C., found at Falerii, has been compared by Nougayrol (1955) with the Babylonian livers and he finds them absolutely identical with one another, thus confirming the words of Dhorme (1945), who wrote: "The Etruscan haruspices are the heirs to the diviners of Babel and Assur."

The question of the Etruscan interpretation of lightning will not be dealt with here. According to Diodorus, the Etruscan laws for this were observed throughout the world. The lightning of Tinia is regarded by Pettazzoni (1931) as being of Babylonian origin, but Furlani considers the evidence for this is insufficient.

The Etruscan religion owned a treasury of books; and Oriental religions are characterized by the fact that they are associated with the use of books. Moreover, the Oriental and Babylonian origin of one of these books can readily be ascertained.

In the 6th Century A.D., Johannes Lydus wrote a treatise, *de ostentis*, containing the Greek text of a brontoscopic calendar, ἐφήμερος Βροντοσκοπία, attributed to the mythic hero Tages, and translated by Nigidius Figulus, a contemporary of Cicero. The object of this calendar is the explanation, for every day of the year, of what is foretold by thunder, e.g. revolutions in the towns, the fall of kings, war, disease and famine. The authenticity of this text has been questioned by Wachsmuth and by Swoboda, who explain it in terms of Byzantine history. On the other hand, Bezold and Boll (1911) and others recognized that the model of the brontoscopic calendar is found in the Babylonian text of the books of Nebuchadnezzar.

In addition to what has been said by these scholars, my own

suggestion (Piganiol, 1951) is the following. We read in the work of Cicero a speech called *de haruspicum responsis*, which deals with the interpretation of thunder heard in the Roman countryside. Cicero gives us the text of answers made by the haruspices, and this text is the exact counterpart of the Etruscan calendar. In fact, we read in the calendar that thunder heard on the 25th September foretells dissension between powerful people and progress towards monarchy, διχόνοια ἔσται τῶν δυνατῶν, ἐκ διχονοίας τῶν κρατούντων εἰς ἕνα τὴν πάντων δύναμιν ἐλθειν. It is possible that thunder heard in the year 56 B.C. was heard precisely on the 25th September. The haruspices warned of coming danger: *ne per optimatium discordium disensionemque pericula creentur . . . qua re ad unum imperium provinciae redeant.* It is clear that the Etruscan calendar handed on to us by Lydus is identical in structure with the Babylonian text, and with the text of the books of the Roman haruspices.

A strange omen, worthy of study, is that of a ram with purple and gold wool, which foretold the happiness of the emperor. This belief should be common to the Etruscans and Babylonians (Clemen, 1949).

The names of the Etruscans and the Chaldeans were again associated in a text by Theodoretos, *Graecorum affectionum curatio* X, 3 (Migne, P.G., 83, 1061), which mentions the obscure caves where Tyrrhenians and Chaldeans evoked the souls of the dead: Ἐν δε τῇ Τυρρηνῶν καὶ Χαλδαίων τῆς νεκυομαντείας τὰ ζοφωτατα ἄντρα. It is true that no records of such places of worship are found in archaeology. However, on studying the Church of S. Maria del Parto in Sutri, Cumont tended to the belief that this type of subterranean sanctuary could be explained on the basis of religious ideas originating in Asia Minor.

There remains the Etruscan conception of the next life. Etruscan tombs may be divided into two main types: (1) tombs surmounted by tumuli, whether the sepulchral chamber is excavated out of the ground or built up with stones; (2) rock tombs,

which are hewn out of rock, with sculptured façades. Both types have been found in Asia Minor. The first seems to be typical of Lydia; under the tumulus of Sypiloe there is a room which is exactly like the famous Regolini-Galassi tomb, and the tomb of Alyatta near Sardis encloses galleries and rooms under a vast tumulus. The second type seems to come from Phrygia, although the decoration of the Phrygian façade is not in the style of the Etruscan tombs. Thus, in the case of the tombs, comparison leads us not to Babylon but to Asia Minor.

The monsters depicted in paintings in Etruscan tombs would seem to have their origin in Mesopotamia. This hypothesis has been put forward, but with great caution, by de Ruyt (1936), in his book "Charun, Etruscan Devil of Death". The Etruscan devil *Tuchulcha* may be compared with the Chaldean devil *Labartu* which, however, is a female. De Ruyt ends by asking: "Is this a question of a remote influence, explained by commercial and artistic relations during the Orientalizing period, or is it a memory of their Oriental fatherland retained by the Etruscan emigrants?" This reasoning does not convince Professor Luisa Banti; and I should add that Charun has no counterpart in the monsters of Asia, whereas he was known to the people of Gaul. Who can say whether the Etruscan Charun, who was not found in any picture before the 4th Century B.C., had not been taken from Celtic mythology?

It cannot be denied that there is a close relationship between the Etruscan religion and those of the Orient; on the other hand, there is almost no link with the other religions of Italy. Cicero was right when he classed the Etruscans with the barbarians: *vos Tusci et barbari* (*de nat. deor.*, II, 10).

One objection that comes to mind is that most of the texts that we have studied are from a very late period, e.g. those of Johannes Lydus, Suidas and Theodoretos. As already mentioned, the Piacenza and Falerii livers do not date from before the 4th, or at most the 2nd Century B.C. It is true that interest in Etruscan

matters increased during the late Empire: Etruscan haruspices were among the philosophers who accompanied the Emperor Julian on the expedition to Mesopotamia. We must also take into account what was said by scholars of Etruscan religion, from the end of the Republic and afterwards, among whom were Nigidius Figulus, Tarquitius Priscus and Fonteius Capito. But I do not think that this objection is valid. The testimony of the brontoscopic calendar is clear: the Etruscans adopted the science of the Orient and particularly of Babylon. As to how the Babylonian civilization reached the Etruscans, I do not believe that the Etruscans came from a limited region of Asia Minor: some came from Lydia, some from Mount Taurus, others from the coast of Euxinus Pons. An Asiatic emigration took place at the same time as the Greek emigration. It has already been observed that the legend of a simultaneous arrival in the West of Greeks and Trojans expresses in the language of mythology the historical fact of the arrival in the West of two different enemy groups.

The fact that the Etruscan religion was an original one, closely related to the religion of Mesopotamia, seems to me to constitute the most decisive proof of the Oriental origin (see Piganiol, 1953) of the Etruscans, i.e. those colonists who, at the end of the 8th Century B.C., overcame the more ancient populations, already composed of various elements, who were settled in Tuscany.

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THE SCIENTISTS' CONTRIBUTIONS TO ETRUSCOLOGY

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"The problem of the reconstruction of the physical conditions of the Etruscan coast in ancient times is linked, on the one hand, with that of the archaeological map and, on the other, with the ancient vegetation of the region. The archaeological map is only a first step towards the making of an historical map, which should correspond as far as possible to a modern one, in which the anthropogeographical conditions are projected on to the corresponding physical background, i.e., on to the outline of the coast, water and land of each region."

This observation of Marinelli (1927) clearly indicates how, for one single branch of naturalistic research, the collaboration of many branches of science is indispensable: topography, terrestrial morphology, botany, forestry, geography, thalassography. Thus, we can imagine what a wide collaboration is required to face all the problems of the reconstruction of the environment and activity of the Etruscan people in their seven centuries (at least) of existence, with which aim the Institute of Etruscan and Italic Studies was founded by Professor Antonio Minto.

The following is a summary of the results obtained in the thirty years of life of the Florentine Institute, and which have been published in 25 volumes of *Studi Etruschi*.

In the field of geophysics, just as the deltas of the Tiber, the Arno and the Po (which is interesting also because of the commercial centre of Spina situated where the river flows into the

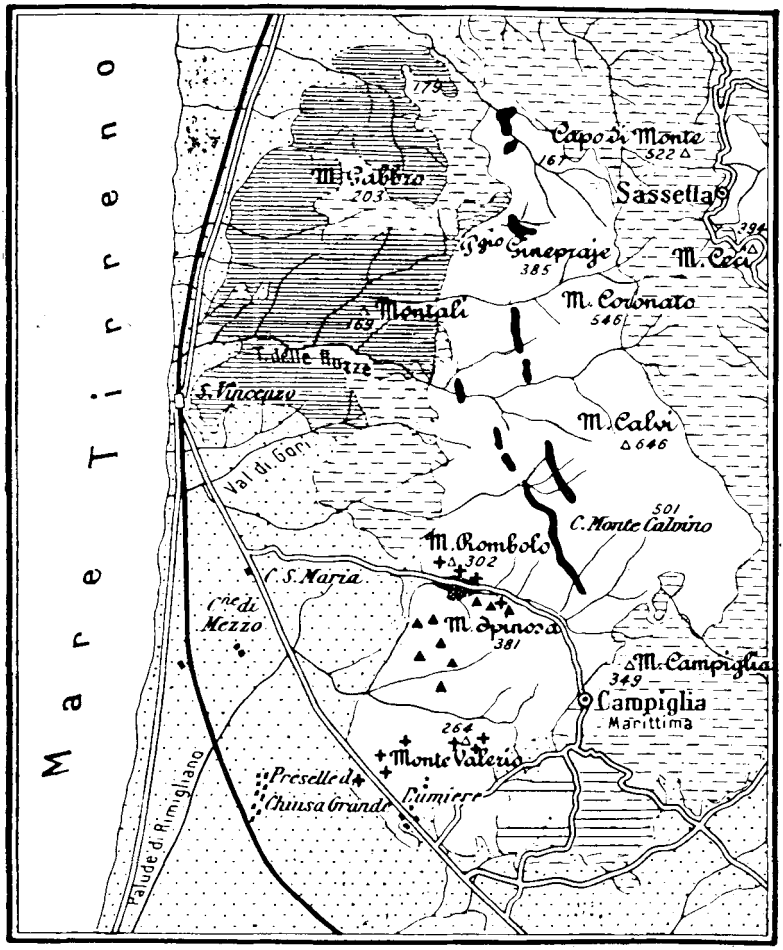

FIG. 1. Geologicomineral map of the Campigliese region. Scale 1 : 100,000

quaternary and recent. trachyte.

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FIG. 2. Furnace found in Val Fucinaia: photograph.

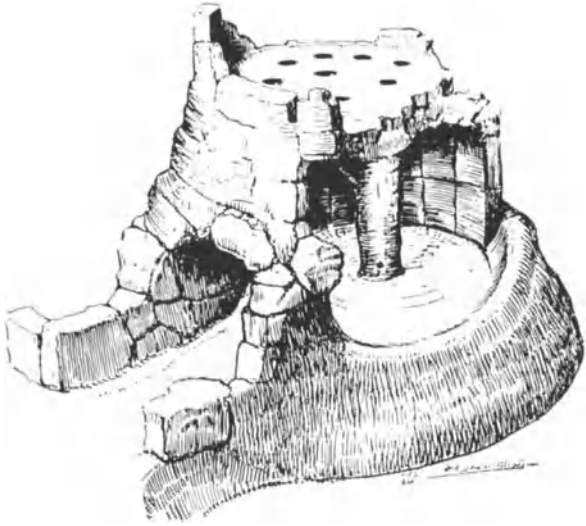

FIG. 3. Furnace found at Val Fucinaia: diagrammatic representation.

Adriatic Sea) are perhaps the most important in the world for the history of natural transformations of the earth's surface, so are the marshes of Etruria on the Tyrrhenian coast. Very interesting results have been obtained in a physicohistorical study of the coast-line from the hills of Leghorn to the Magra, the bed of Lake Bientina and the marshes of Fucecchio, in comparison with the deltas of the Arno and Serchio (Tonioli, 1929).

The natural surroundings in which the Etruscans lived may, in general, be well defined by means of a comparative study of the literary sources and of the evidence of archaeological material which has been sifted and analysed from various points of view—botanical, mineralogical, zoological, irrigational, toponomastic, climatic, nutritional and so on (Bonacelli, 1928).

The exploitation of mines in Etruria is particularly interesting, since it led to the growth of very important centres and most profitable trade (see Neppi Mòdona, 1955; Cambi, 1957); for instance, the mines of the Island of Elba furnished for centuries the raw material for use in the metallurgical centre of the district of Populonia, where mounds of hard cemented dross were formed. From this came the idea of an archaeological map showing the exploitation of ancient mines in Italy, later taken up by UNESCO and extended to the whole of Europe, but first conceived in the Section of Natural Sciences in our Institute, the section which is directly concerned with the technical contribution of scientists to Etruscology. For this purpose three types of questionnaire were sent out, concerning (*a*) objects pertaining to mineral or metallurgical industries; (*b*) traces of ancient mine workings; (*c*) traces of ancient metallurgical industries. Special research was undertaken with very good results in the valley of Fucinaia, in various metalliferous mountain zones of the Campigliese, in the district of Heba, and elsewhere (Fig. 1) (Minto, 1948–49, 1954). In the valley of Fucinaia were found a number of furnaces, some of them very well preserved (Figs. 2 and 3). Built in the form of a truncated cone, and lined with refractory tiles, they were divided into

two chambers by a perforated partition supported by small columns of quartziferous porphyry. The perforations in the partition, which were encrusted with copper carbonate, were closed by pebbles of white crystalline limestone. The lower chamber had a square opening, reinforced with blocks of porphyry, and with a retaining wall of the same stone. At this opening the serving trench ended. In these furnaces, the ores from the local strata were processed. Examination of the residue from the process shows that the degree of efficiency was very low, and recently the residue has been used to advantage, in industry. The coal was studied also, and was found to have come from the species *Quercus cerris* and *Pinus aucuparia.* The furnaces were used mainly for copper ores. The upper chamber of the furnace was filled with pieces of ore and coal; and some quartz and silicon material added to separate out the iron. When the furnaces had been loaded, a fire was lit in the lower chamber; iron oxide separated out and was collected in the upper chamber, while the molten copper passed through the perforated partition and was collected in the lower chamber. Miners' lamps have also been found.

In general, it can be said that near Campiglia and elsewhere were found ores of iron, copper, argentiferous lead and tin. The important metallurgical centres apart from Elba were the regions of Massa Marittima, Volterra, Follonica, Mount Amiata and the Apuani mountains.

Analysis of the building material of tombs at Populonia showed that it was coastal and sandy panchina and lake travertine (Blanc, 1935).

For their sculptures the Etruscans used local stone—various kinds of limestone and volcanic rocks (travertine, sandstone, alabaster).

Etruscan objects in bone and ivory, in the Florence Museum, have all been examined, and most of them proved to be made from compact bone material of large mammals, and from the horns of deer.

The examination of bones and, in particular, skulls, was one of the earliest concerns of our Institute; and questionnaires were sent to all museums and institutes in order to establish how much material was available originating for certain in Etruria. However, such certainty was often lacking, and the material was not always in good condition. Furthermore, the fact that bones have come from Etruscan territory does not always prove that they were bones of Etruscan people; besides, life in the territory of Etruria dates back to prehistoric ages. Its cultural background is indeed considered one of the oldest of the Premediterranean European *facies* (Mochi, 1927; Blanc, 1935).

The variation in skulls is very great and, therefore, they can tell us very little about the ethnic complex of the Etruscan people. Among them are dolichocephalic and brachycephalic skulls. It is not improbable that one characteristic feature of the Etruscan skulls was the position and size of the occipital fossa relative to that in the Roman skull; a minor development of the epiphysis styloidea may have been another; and there may have been various other specific characteristics. On the other hand, an accurate and complete examination of numerous skeletons proved that the Etruscans were of medium height (Puccioni, 1927; Cipriani, 1929).

Besides the research on Etruscan skulls undertaken by our Institute, studies have been made by other workers with interesting results. Pfennensteil (1954–55) examined a great number of skulls from Tarquinia, which included mesocephalic and dolichocephalic ones; and compared them with others in Rome and in Florence, concluding that many of these skulls show the characteristics of the type *Grazilmediterranide*, of the group *Kokon*. On the other hand, Gerhardt (1954) examined about 1,000 heads depicted on lids of urns and sarcophagi of the 2nd–3rd Century B.C., but had difficulty in obtaining reliable anthropological results because of the bad state of preservation and the porous nature of the material, lack of light, and other factors. However, he

was able to draw some negative conclusions: for instance, that these heads are not of the Alpine type, as indicated in a recent map of racial expansion in Europe, which describes the Etruscans as Alpine and Mediterranean. Instead he suggests, with some reserve, the cromagniform type, which he calls the "Rocchi" type, because he found a similar type in a peasant family of that name, living in Etruria; and this type is depicted on many urns from Chiusi and Volterra (Fig. 4).

In a study of the vegetation in Etruscan times, one must bear in mind that the initial organization of the society and state of Etruria coincided with the first anthropic lay-out of a great part of the local vegetation. Two types of vegetation can be associated definitely with the Etruscan age in Tuscany, i.e. the Mediterranean type (bushes) and forests of beech and fir on the mountains (Negri, 1927). From the representations of plants in the decoration of tombs (paintings), mirrors (bronze), and *cippi* (stone), and allowing for the limited value of these ornamental motifs, the following have been recognized (Fig. 5) (Pampanini, 1930, 1931): artichoke, convolvulus, crocus, acanthus, laurel, cypress, ivy, corn, lily, pomegranate, apple, date, olive, dwarf palm, poppy, pine, oak, rose and vine. Of these only five are indigenous: artichoke, dwarf palm, ivy, oak and convolvulus. The others are of Oriental origin. Furthermore, analysis of coal used in the Fucinaia Valley showed that in its vegetable origin it belongs to the genus *Quercus*, species *robur* L. (subspecies probably *sessiliflora*) and species *cerris* L. (Tangiorgi, 1937). Analysis of a wooden sarcophagus from Falerii, in the Florence Museum, showed that it was made from the wood of a dicotyledon, i.e. an oak, probably *Quercus cerris* L., which is the characteristic oak of the Apennine highlands (Fasolo, 1935). Other samples analysed were made from *Fagus silvatica* L., *Ulmus campestris* L., *Rhamnus cathartica* L., *Pinus aucuparia* Erh., *Salix* sp. and *Quercus suber* L. Various samples of wood from Vetulonia (Fig. 6), mostly in the form of points or cusps of lances, studied in transverse and, where

FIG. 4. Heads from Etruscan cinerary urns.

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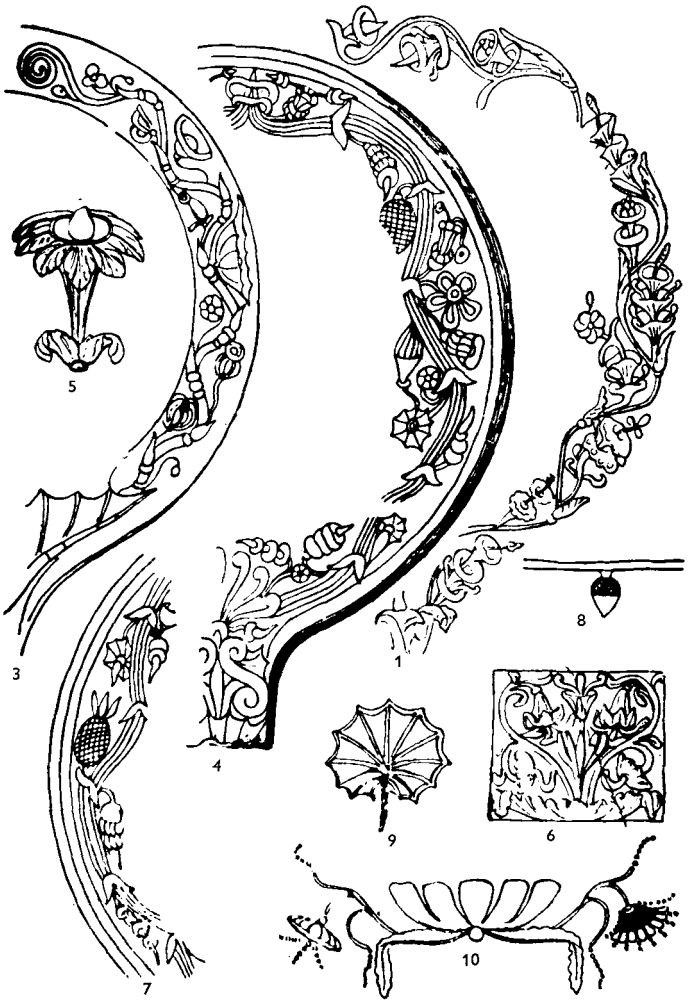

FIG. 5. Details of floral ornamentation on bronze mirrors (1, 3, 4, 7, 8) and cinerary urns (5, 6); cf. detail from Mycenaean tomb (9). The Etruscan origin of the ornamental detail (10) is uncertain.

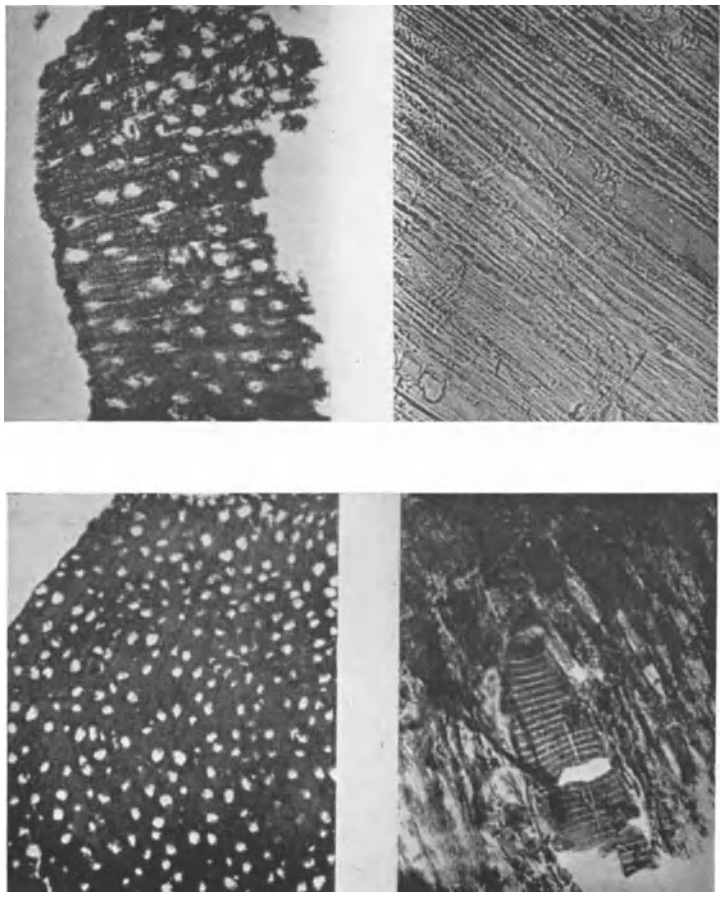

FIG. 6. Samples of wood from objects found at Vetulonia. Upper left: transverse section (diameter 100); upper right and lower left: tangential (diameter 150) and transverse (diameter 50) sections of a second sample; lower right: scalariform apex seen in a third sample (diameter 500).

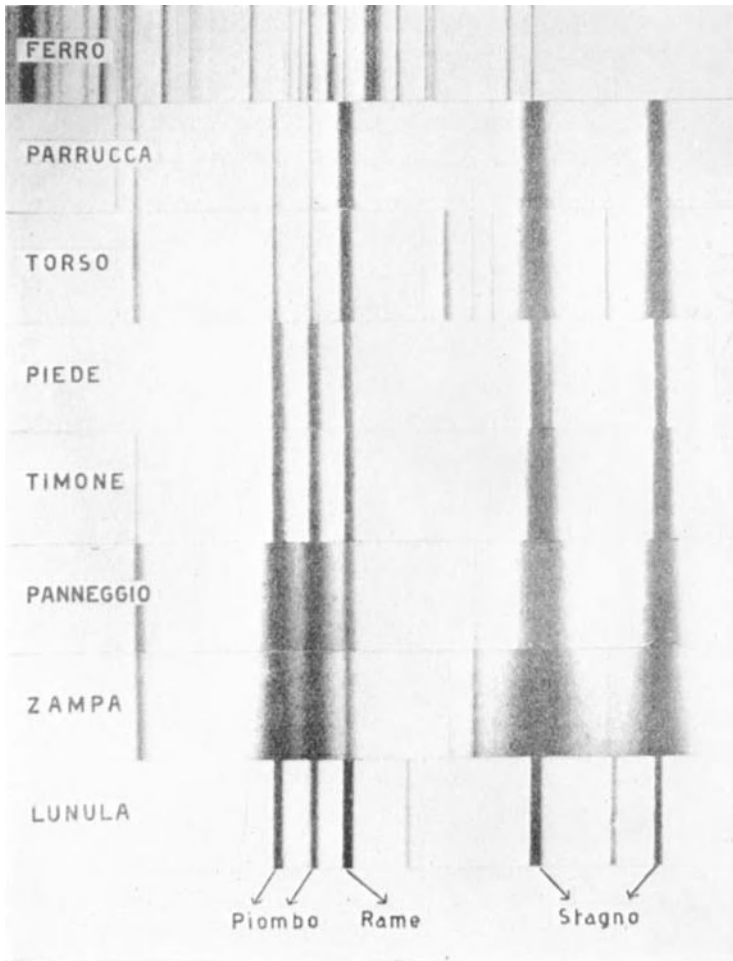

FIG. 10. Spectrographic analysis of group of votive bronzes from Chianciano (Chiusi).

possible, in tangential and radial section, were found to belong to the genera *Acer*, *Viburnum*, *Crateagus* and *Pinus* and to species common in Tuscany (Fasolo, 1938). From Marsiliana d'Albegna came pieces of *Quercus cerris* and *robur*, *Erica arborea* L., angiosperms and *Rhamnaceae* (Fasolo, 1940). A case from Bolsena and a comb from Corchiano were made of *Buxus sempervirens* L., which is common in Tuscany (Fasolo, 1941). An ebony statue of Atargatis, from Arezzo, was made of *Diospyrus*, an Asian genus in keeping with the Syrian–Oriental character of the cult image (Fasolo, 1944).

As regards the agricultural tools used by the Etruscans, firstly there is the well known plough from Talamone (Grosseto), which greatly resembles the ploughs used in Tuscany today; and votive tools from Talamone correspond with various agricultural implements (Vitali, 1928, 1930, 1931, 1933). Also found were a very primitive plough in the form of a hook, a ploughshare, a hoe with a handle, a hoe with two teeth, a hoe with a very thin blade and a short handle, a pickaxe, two yokes for oxen and two scythes for corn. Agricultural tools made of iron, from Luni, which are now in the Florence Museum include long- and short-bladed hoes, a scythe for corn, various pruning-hooks, an axe, bill-hooks and sickles. From Talamone, also, came iron tools, such as hoes, large and small scythes and pruning hooks.

Physicochemical methods have proved very valuable in that only minute amounts of material are required for analysis, thereby minimizing the damage to objects or fragments under study. Spectroscopy and examination by means of ultra-violet light are particularly useful.

Bronze mirrors from various sources were examined (Grassini and Piccardi, 1933). Covered originally with silver or gold, these mirrors demonstrated a very high degree of workmanship.

An iron knife from Volsinii (Figs. 7–9), dating from the 3rd Century B.C., on microscopic examination was shown to consist of iron covered with perlite (Panseri, Garino and Leoni,

1955). Metallurgical analysis of this knife led to interesting hypotheses as to the method of its manufacture.

Various bronze objects, such as nails for wood, small statues and swords, have also been submitted to metallurgical analysis.

Polarographic and spectroscopic examination of lead has shown that, in general, it was of a very hard quality, with minimum amounts of copper, bismuth and silver; one sample contained tin and another antimony. A group of seven votive bronzes from Chianciano (Chiusi) was analysed (Piccardi, 1950–51), i.e. a wig, a bust, a foot, a shaft, a horse's hoof, a *panneggio* and a lunula (Fig. 10). The wig and the bust were found to have only a slight content of lead; the foot, shaft and lunula have a fair content of lead, but the shaft contains very little tin. The other pieces are very rich in tin and lead. An important observation is that some of the samples contain cobalt and sometimes nickel, which suggests a Spanish origin, since nickel and cobalt do not occur in Italy. A detailed examination was made of copper nails from a tomb in Tarquinia. These probably belonged to a wooden sarcophagus (Speroni and Cozzi, 1939). The chemical composition, mechanical characteristics of the material, technique of manufacture and action of the subsoil were studied. Examination of a bronze vase from Vulci showed a distinct difference between the bronze of the handle and that of the vessel: the former is an alloy of copper and tin, the latter an alloy of copper, tin and lead. This may be due to different techniques of production. The handle was soldered to the vessel with an alloy of equal quantities of tin and lead (Panseri and Leoni, 1955–56).

In determining the authenticity of ancient bronzes it is very important to be familiar with the type of patina found on them. Bruno Bearzi, a great Florentine craftsman in bronze, declared that bronze exposed to the weather acquires an amorphous green surface patina under which is formed a very thin and lasting layer of "noble" patina; the latter must never be removed since it is this which gives an antique appearance to the object (Bearzi,

1950–51). Various electrolytic methods have been used successfully to remove the patina from coins, brooches, mirrors, heads of lances and iron knives. Chemical analysis has been carried out on 627 silver coins from Populonia (Speroni and Mori, 1950–51): most of them have a specific weight of 9·5–10·5; coins without patina have a silver content of 96 per cent, those with copper patina have a copper content of not less than 6–7 per cent; the formation of silver chloride patina indicates that the alloy contains bismuth and antimony. No relationship could be found between the composition of the alloy and the fiscal system.

The Etruscans were famous for the skill of their goldsmiths, particularly the technique of granulation, which modern craftsmen have tried in vain to imitate. The technique of soldering together gold spheres, rediscovered in modern times (Fig. 11), was perfected by the Etruscans.

Spectrographic analysis has been carried out on samples of silver; in one case, for instance, lead and tin, but not zinc were found; in another there was no tin but zinc was present. Therefore, the silver always contains impurities which vary greatly from one sample to another (Piccardi, 1935, 1936). Etruscan amber has been found to contain siliceous cell walls of the microscopic unicellular algae, *Diatomaceae*. Three types of amber have been identified and can be compared with amber from other sources (Zanon, 1929).

Pottery of different kinds has been examined, from the Neolithic and Villanovan to the very late Etrusco-Roman. Analysis was done as with cements, compiling long numerical tables of molecular ratios. In general, it can be said that the first material used was quartziferous sand, feldspathic, argillaceous, and insufficiently baked. On the other hand, examination of late coralline vases from Arezzo shows that boric acid was an essential component of their varnish (Nasini and Grassini, 1932).

A number of objects have been analysed as a matter of interest, e.g. a cushion made of balsamic resin, from a tomb at Populonia.

It was intended for burning and may have been a resin of myrrh (Passerini, 1934).

The composition of the paints used and the technique of their application have been studied. They were prepared from local earth (vermilion from the mercury mines of Mount Amiata, blue and green from the silver and copper mines of Chiusi, etc.); raw material must have been imported for making light red, deep blue and bright green (cerulean may have come from Cyprus, and almost certainly from the Orient). Various fragments were submitted to chemical analysis (Branzani, 1933; Grassini, 1934, 1936). The intensity and variety of the colours in the painted tombs of Tarquinia are now represented there by only a few of the original paintings; some were quite ruined and others were moved to Rome and their restoration attempted with little success. Fortunately it has been possible to make very good reproductions of them using modern techniques of photography and printing (see Leisinger, 1953). Etruscan painting achieved its finest expression in the tomb of the Baron, around 500 B.C.

Some zoological data have been obtained through the study of tomb paintings and sculptures of various kinds. Baldasseroni (1929) concluded that very few animal species were represented in the paintings, and are shown repeatedly in the same attitude. All the species are still extant; most of them are typical of Italian fauna, some are African. Among the mammals, carnivores are frequently depicted (lion, leopard, fox and perhaps ferret); domestic animals including a dog, cat, mouse and hare. Deer are stylized; dolphins are shown but not very realistically. Birds include wild duck and perhaps king mallards (tombs of the Hunter, the Fisherman and the Lioness, at Tarquinia). In the tomb of the Priests was found a web-footed bird, a cormorant; in the tomb of the Tricilinium, a cock, a hen, a blackbird and a thrush; and in the tomb of the Inscriptions a bird of prey (probably an owl). Also found were reptiles (ophidians) and, in the tomb of the Lioness, diving sharks.

The monkey merited special attention; and even had the honour of giving its name to the tomb of the Monkey at Chiusi, where it appears to great advantage, tied with a little chain; but already much earlier, during the 5th Century, Etruscans were decorating bronze, electron and amber objects with the monkey motif (Bonacelli, 1932) (Fig. 12), and it is also found in paintings in the tomb of the Seven Chimneys at Orvieto. This animal was certainly introduced by the Carthaginians, whence the name: *armi* = Etruscan *arimos* = flat-nosed animal = *simus* = *simia* or monkey.

The food of the Etruscan and Italic peoples, since prehistoric times, can be readily identified. Abundant evidence was found in the grottoes of Belverde on Mount Cetona near Chiusi, dating from the Bronze Age (Oliva, 1939): cereals, leguminous and other vegetables, various fruits. Thus it has been established that corn was cultivated during the Bronze Age, and very occasionally the type *Triticum sphaerococcum*, which was known in parts of Neolithic Italy, but the fact that it is never mentioned in ancient literature indicates that it disappeared from central Italy in the last three centuries B.C.

This very brief summary of scientific research in the field of Etruscology carried out during the last thirty years demonstrates the great amount which remains to be done by workers in all branches of science. The Institute of Etruscan and Italic Studies in Florence continues with its programme of research, in collaboration with the metallurgists of the Italian Association of Metallurgy and the Institute of Light Metals in Milano-Novara.

Although linguistics cannot offer a satisfactory solution to the problem of the Etruscans, in this field too the expert can learn much by scientific investigation. Now, as the result of this meeting between archaeologists and glottologists on the one hand, and biologists, anthropologists and geneticists on the other, new possibilities are opened up for a collaborative programme of research, to which the Florentine Institute is ready to give moral

support by publishing in *Studi Etruschi* the contributions subsequent to those published in this symposium.

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Medical Biology and Etruscan Origins
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FUTURE RESEARCH ON THE ORIGIN OF THE ETRUSCANS

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WHEN I first saw how the programme of this meeting had been formulated by the distinguished people who have organized it, I was glad to have been given the opportunity of meeting scholars who were neither archaeologists nor historians, but were interested in solving the problem of the origin of the Etruscans, and were ready to help. I thought that a new method of research might come out of this meeting, a method that might reach a solution which all scholars could accept. Also, if new branches of research were called upon to help, this meant that the organizers were not satisfied with existing theories and recognized that historians and archaeologists were unable to go any further.

I wish to underline that this is a new way of looking at this problem; also that it is an honest way of summing up our present position. If we want to achieve results, if we want this meeting to be a step, and an important one, towards a solution, we must face an unpleasant fact: every single theory that has been formulated meets some of the existing archaeological, historical and philological data, but not all of them.

The theories on the disputed question of the origin of the Etruscans have been already explained by Mr. Cook in his introductory address. Ancient theories may be reduced to two. (1) The immigration theory: the Etruscans came from Lydia, by sea. Our authority for this is primarily Herodotus. (2) The autochthony theory: the Etruscans are native to the country, they always lived in Etruria. Our authority for this is Dionysius

of Halicarnassus. Modern scholars added a new immigration theory: the Etruscans came to Italy from the north, across the Alps.

If we had only three theories on this subject, we could still feel on safe ground. But scholars have accepted from ancient sources only the bare fact of migration or autochthony. They do not agree on details. There are many theories; almost as many as there are scholars who have treated this subject. Each theory divides into many subtheories and each subtheory again divides into many subsubtheories. Let us examine the eastern immigration theory. Ancient tradition disagrees on many details, but mostly on the route followed by the Etruscans. Modern scholars disagree on every single detail given by Herodotus. They not only reject fantastic details, such as the eighteen years of great famine, the Lydians trying to meet it by taking food on alternate days and occupying themselves with inventing new games in order to forget they were hungry. They also change the date of the migration. According to Herodotus, it happened at the time of the Trojan war (12th Century B.C.). Only two scholars, Weege and Berard, have accepted the date given by Herodotus. Some scholars place the Etruscan migration at the beginning of Oriental influence in Etruria. Others believe the Etruscans to have arrived in small groups and at different times; the earliest group to have landed about 1000 B.C. Even the place where they landed is uncertain: some say Etruria, others the western coast of Italy. But this is to expose the subject in its most general and simplest form. There are many minor details on which scholars do not agree; I cannot repeat them all.

The same lack of agreement exists for the theory of autochthony and for the theory of a migration from the north, a proof that none of these theories is really satisfactory. Each leaves much that is unexplained, or that may be interpreted differently. Attractive as some theories are, there is not sufficient weight of evidence to support them.

If the Etruscans were native to Etruria, then we must identify them either with the Neolithic or the Bronze Age settlers. This seems impossible, because the Neolithic and Bronze Age inhabitants occupied a smaller area than that occupied later by the Etruscans and none of their settlements coincided with later Etruscan towns. Moreover, there is a break in the culture of Etruria at the beginning of the Villanovan culture. If the Etruscans came from the north, we must identify them with the bearers of the so-called Villanovan culture. But why, then, are the earliest Villanovan settlements in southern Etruria? We ought to find them in northern Etruria. If the Etruscans came from the east, when did they arrive? If does not seem likely that they came at the beginning of the Orientalizing period, because there was very little real Oriental import at the end of the 8th and in the 7th Century. What we have is mostly what was also imported into Greece. Oriental connexions are strong, but much comes in at second hand, through Greece: the chimaera for example, which has the double schema adopted from Protocorinthian painting, and the animal frieze. There is almost as much Greek influence in 7th-Century Etruria as there are Oriental influences. Moreover, what Oriental imports we have are of the wrong kind, because we have no pottery. We find nothing that cannot be explained by trade connexions. Also, if the Etruscans arrived in Etruria in the second half of the 8th Century, why did they drop their own alphabet in order to pick up, almost at once, a new alphabet, a Greek one? Why did they drop their own superior civilization and art—i.e. their own architecture, sculpture and pottery, their way of building and decorating—why did they drop all this, in order to adopt the simple life of the Villanovan settlers? Had they left Lydia because they wished to live like hermits? If we follow Herodotus and place their migration in the 12th Century, that must have been when Etruria had a Neolithic or Bronze Age culture; they would have had to adopt life in rock-shelters, caves and huts. They might have come at the beginning of the

Villanovan civilization. But we are again confronted by the same problem: they dropped everything they had been used to in Asia Minor in order to lead the simplest of lives. There is no evidence of a superior civilization at the time. At Lipari, where the Mycenaeans only came from time to time in order to fetch obsidian (they never really settled there), enough Mycenaean sherds have been found to prove their presence.

If we turn to philological evidence, we find that it is even more uncertain. We do not agree on the character of the Etruscan language, which has been compared to almost all ancient languages and even to some modern ones. As for the Etruscan gods and Etruscan religion, I cannot share the general optimism. We have nothing but lists of names; we know absolutely nothing about the gods themselves. We are not even able to agree if words which have the root *tin-* are to be interpreted as meaning the god *Tinia*, the Etruscan Jupiter, or if the word means "day". As to the *Etrusca Disciplina*, we ought to see first if nothing has been added to it later on, during the Roman Empire.

I only wished to underline how uncertain our theories are and how contradictory our sources. I confess my own ignorance: I do not know who the Etruscans were, nor where they came from, when they arrived and if they arrived. We appear to have reached a dead end. This is why I like the idea of getting outside help in an attempt to reach a solution. A solution, a real solution, which may be accepted by everybody, can only be reached if, instead of twisting the limited data we possess in order to adapt them to existing theories, we honestly recognize our own ignorance of the origin of the Etruscans. If we want this meeting to be successful, if we are to map out a method of future research in collaboration with the distinguished scientists who are working in the biological field, we must first recognize that on the origin of the Etruscans we can say nothing but *non liquet*.

Cook: It will be important for us to learn now how the biologists can use our material rather than to discuss the relative merits of our own theories. The biologists will have noticed that the archaeologists are not agreed on the Etruscan question. This, however, is one of the most peaceable discussions that I have heard so far on the Etruscan problem.

Ceppellini: As a biologist I should like to hear some more details from the speakers we have just heard. We have obtained much important information, but none, so far, about a possible census of the Etruscans. I do not expect a precise answer, but at least it would be interesting to have some idea of the size of the Etruscan towns. Did the Etruscans live mainly in towns, were they traders, or did they live also in villages as agricultural or pastoral communities? We biologists need this information.

As for disagreement, I think the archaeologists will find that the biologists also disagree very much!

Banti: It is impossible to estimate the size of the towns. We are not even certain whether all the known centres were villages or real towns. In northern Etruria, the people mostly lived on farms, more or less in agricultural centres. In the south and along the coast from Vetulonia down to the Tiber they lived probably in towns; but I do not believe that anyone can be certain about it.

Cook: It would be of some help to give outside and inside limits. Dr. Foti, surely you could give us an idea of the size of some of the cities of southern Etruria?

Foti: We have very little definite information on the subject of the population of the cities. We must not forget that when we speak of cities and towns, we are referring to periods during the 4th Century B.C. We know that the area covered by Cerveteri and by Vulci was about 150 hectares. The area was so extensive due to the fact that the inhabitants of these towns had to defend themselves against the danger of enemies, such as the Romans and the Celts; therefore, everything that was necessary for their way of life had to be included within the

perimeter of their towns, which were centres of collective living, rather than towns as we know them. The area covered by 150 hectares thus included gardens, orchards, space for cattle, and so on.

One might try to estimate the actual size of the population from the number of cemeteries attached to the inhabited centre. Whereas an actual town extended over 150 hectares, cemeteries extended over 600 hectares. We know also that, for instance, the necropolis of Cerveteri covered the period from the 7th to the 2nd Century B.C. I think that it would be possible to estimate the number of inhabitants, because we know that in Etruria the family tombs contained only one or two generations, and that there was very little cremation; therefore, one could calculate the number of inhabitants belonging to one given generation. However, this has not yet been done.

Ward Perkins: On the question of population, perhaps Prof. Banti is unduly pessimistic. Although archaeology may for the moment have reached a dead end to the extent that it cannot produce an answer, the fact remains that, in a great many fields, archaeological method is changing rapidly and some most promising new lines are emerging. One, for example, is that at last, instead of looting tombs, we are beginning to turn our attention to the cities. There are several very promising excavations; the one which is most likely to yield results of an interesting and definitive character is the current Swedish excavation at San Giovenale where a relatively small Villanovan settlement, which subsequently continues into the Etruscan period, is being excavated more or less in totality. That will give us a clearer picture of the interior of one of these towns than any amount of word-spinning at the present moment can do. However, in attempting to estimate the population, consideration may also be given to the topographical approach being tried at the moment by the British School at Rome, i.e. that of taking a particular area and trying to plot the settlement at successive periods. So far we have worked out in considerable detail one area, that of the Faliscan territory, and we also have a good deal of data on the territory of Veii. In both of those the impression gained is that, allowing for inevitable changes in detail and in emphasis, the picture of settlement at the height of the Etruscan period was probably very similar to the picture that one would find, for example, in the 12th, 13th or 14th Centuries. The pattern of the road systems

is something the same; the actual settlements are very alike, with one possible important difference, that the majority of the mediaeval villages were probably rather more closely settled than those of the earlier period. If any figures on the mediaeval Italian population are available, one would probably not be far wrong in deriving a picture from them of what one would find in the same area, at any rate within certain defined areas, in southern Etruria.

Bullough: Taxation records of the big mediaeval towns do exist which can provide an estimate. Pre-Black Death Florence, for example, at its height had an urban population (town and *suburbio*) of circa 55,000, and its rural population was perhaps another 50,000 (though where they drew the line between suburban and rural is not quite clear). At the end of the 12th Century the urban population was probably under 20,000; 20,000 or 30,000 is much too high for an Etruscan town—10,000 is much more likely.

Cook: I would suggest that the figure for an Etruscan town can hardly be less than 5,000; and there were at least twelve of them. You could assume, therefore, that a population of 60,000 would be the minimum. This would indicate the region of figures in which you are working.

Bullough: I would put it higher. I would put it at 100,000 or 150,000 for Etruria.

Ward Perkins: I find it very hard to believe that a city like Tarquinia did not have more than 5,000 inhabitants, and in addition to the towns there were a number of quite densely settled rural areas. My own guess (and it can be no more) would be a figure for Etruria as a whole in the order of 200,000.

Strong: Are we not given certain figures (they may be quite unreliable) for the troops involved in the siege of Veii, for instance?

Ward Perkins: Livy, I think!

Ceppellini: Were all the people, or only the aristocrats, buried ceremoniously in the tombs?

Foti: They were democratic as regards burial; everyone was buried in family tombs. Inside were the actual members of the family, and in the outer rooms were the servants. In other words, each tomb combined two or three burials, all belonging to the same family (in the sense of the Latin word *familia*).

Morganti: What is the approximate number of skeletons which has been found? This information could give us an idea of how many inhabitants were buried in a given tomb. From the biological point of view, it is important for us to know how many skeletons we can rely on for our investigations; even an approximate figure, e.g. between one and two thousand, would be useful. Furthermore, it should be possible to make a distinction between the bones of different individuals, for instance to take a particular bone from each individual, so as to avoid the confusion of having a number of bones from any one skeleton.

Foti: Those who excavated tombs from the 18th Century onwards never bothered to count or preserve the skeletons; they were interested only in the objects found. We have just begun to remedy this: two years ago we excavated forty tombs, and we have about forty skeletons as a result. Now we archaeologists would like to ask you biologists: what kind of bones are you interested in, and how should we collect and preserve them? It may be that after one or two years in contact with air, excavated bones will be useless for blood group determination. Do you want the whole skeleton, or is just a bone or two sufficient? We are quite willing to provide you with any material we possess which may help in your research. Do you want details of the site where the bones were found, i.e. whether in clay or chalky soil, in dry or in damp tombs? We know, for instance, that bones found in contact with humus are not suitable for isotope tracer work using ^{14}C. Naturally, we cannot excavate a whole necropolis at once, we must go slowly; but if we archaeologists are to help you, the biologists, you must tell us what you require and we will do our best to provide it.

Oakley: We can certainly let you know later in this symposium what our requirements are for blood group studies.*

Ciotti: To return to the question of a working method in archaeology, I agree with Mr. Ward Perkins that we should take one region, one certain settlement, study it in all its aspects and record every possible observation. It is up to us Italians to do this work, and an agreement should be reached between the Superintendencies and the Foreign Schools working on excavations in central Italy. Some con-

* See appendix I, p. 246.

certed action should be taken. We should keep one another informed so that we might follow the same line of research and our data would then be comparable. We should keep the glottologists informed too, because they have their own requirements. We could then supply uniform data to the biologists. We should do this from now onwards, because what has been done in the past cannot really be used. It is enough to go into one of our depôts, and to see how the few bones which we still have are being kept in a disgraceful state. In order to do something useful, we should all get together and agree as to our new working methods and establish a common policy, a concerted working programme, which might be based on a topographical method: we should study district by district from all possible points of view—historical, prehistorical, protohistorical, medical, archaeological, glottological, etc. If each of us were to keep on working on his own, regardless of what other people were doing, we should never achieve any results.

Siniscalco: Dr. Foti, in speaking of the main cities of Etruria you said that at a certain stage the people left the Maremma in Tuscany because of malaria. Is there any indication of a historical nature as to the date when they migrated from the Maremma because of malaria? Did they, in fact, migrate because of the disease, or were they destroyed by it?

Foti: In some cases we observe that a centre of inhabitation has been moved from one place to another or it has disappeared and we find no traces of it elsewhere throughout the Middle Ages. The information is not available beyond a certain point.

Ciotti: We have data for the Middle Ages, but few for the Etruscans.

Banti: Propertius, in the 1st Century A.D., described those towns destroyed and abandoned because of malaria. Propertius says that there were no longer any living souls in the cities. Some people have specialized in the study of malaria in Etruria.

Kirk: With reference to the archaeological discussion we have had today, and in particular to Prof. Piganiol's paper, it seems to me that, on the whole, the speakers whom we heard before Prof. Piganiol tended to agree that there need not have been any external influence—except for an Orientalizing influence such as that which had so great an effect on Greek art in the 7th Century B.C.—to account for what we have always regarded in the past as the very distinctive features

of Etruscan culture, features which must have implied some kind of large-scale cultural revolution. If the Villanovans came down into Italy from the north and then developed a comparatively advanced civilization, and if, in the Etruscan region, with many Villanovan inhabitants, there were particularly strong Orientalizing influences at the end of the 8th Century, then that in itself might account for most of the typically Etruscan cultural features.

The question of hepatoscopy, or the inspection of livers to obtain auguries, has been much mentioned by writers on Etruscan origins. Prof. Piganiol evidently feels that this kind of evidence is conclusive; but what is the date of the earliest augural liver which has been found at an Etruscan site and, secondly, what are the signs that hepatoscopy was carried on in Lydia as opposed to, say, Mesopotamia? In other words, how close does hepatoscopy come to the actual ritual in Asia Minor—where the exponents of Oriental origin want, on traditional evidence, to make the migration start?

Piganiol: The Piacenza liver dates back to the 3rd and the Falerii liver to the 2nd Century B.C. The strange thing is that the Falerii liver was dated because of the objects which surrounded it, and if it had been found alone it would have been indistinguishable from the Piacenza liver. They differ only in the excavation site, and without this one could not have established a different date for the Falerii liver.

I acknowledge Prof. Banti's criticism, but how are we to explain the fact that at a 2nd-Century site in Etruria we found an augural liver which is similar to the Chaldean liver? The brontoscopic calendar based on the liver is absolutely similar to the documents of Babylon. It has the same text, the same culture. It is certain that the calendar is a copy of the Babylonian text, the same text which was being read at the time of Cicero. I have thought about this, and one could say, for instance, that it was those Chaldeans who travelled throughout Italy in the days of Cato, and who were diviners, who might have brought this method with them. It is possible to believe that the late Chaldeans should have imitated the ancient habits of Etruria, but not that they themselves introduced them. The fact remains that from Falerii, in Etruria, in the 2nd Century B.C., we have an augural liver which is exactly the same as the Chaldean liver. At the time of Cicero, the books of the haruspices had the Etruscan calendar as their model.

Kirk: It is also a fact that the Greeks, for example, were using hepatoscopy at least as early as Aeschylus, and became particularly interested in Babylonian astrology as early as the 3rd Century B.C. If it is the case that, in the Hellenistic world generally, Babylonian methods of augury and astrological ideas were spreading, it is quite conceivable that the first appearance of Babylonian-type hepatoscopy in Etruria might have been no earlier than 300 B.C. This is purely a hypothesis, but it is one worth making since some people maintain that the religious similarity is a concrete argument for the Etruscans having had an Oriental origin some 400 or 500 years earlier.

Cook: It might be argued that the Etruscan elements taken over by the Roman religion must have been taken over before 300 B.C.

Strong: Surely, religion is a worse argument than archaeology for ethnic origins. If one tried to make ethnic groups nowadays out of religious groupings one would get some odd results. As for archaeology, it is only when the evidence of material connexions is overwhelmingly strong that we can deduce a movement from one place to another. Such evidence certainly cannot be produced from Italic archaeology. It cannot be produced in the change-over from the Bronze Age to Villanovans; nor in the Orientalizing period, because we know that an Orientalizing period took place in other countries without involving any ethnic change. The slight evidence for eastern imports of Bronze Age date into Etruria can be explained by contacts with southern Italy, an area which had been partly Mycenaenized. None of this gives any evidence for ethnos.

Cook: It will be much better for the scientists to disregard archaeological interpretations, and to approach the problem with an open mind. It is true that most of the speakers here incline to the indigenous side, but I think one could equally well find a number of Etruscologists who will say precisely the opposite. I feel, therefore, that the scientists should not enquire too carefully into how the archaeologists reached their conclusions.

Bloch: We should not over-emphasize the differences between us, the different points of view of history and archaeology. Many points are now fixed, for instance: (1) The beginning of Etruscan civilization occurred in the 7th Century B.C. (2) Although we speak of the Etruscan people, we cannot assume that they came from the same place;

in fact, all the Italic peoples had not yet disappeared. (3) They may or may not have come from the east, bringing with them religious and cultural elements. They may have come as small groups or in one migration. This point concerning a possible Oriental origin is the most disputed one. At least, we do know that if they came from the east this must have happened at the beginning of the 7th Century B.C. It is important that any further approach to agreement on some points, during these few days, should be fully discussed.

Bullough: What is the evidence for an Etruscan religion, in particular the citation of Martianus Capella? Has this really any positively constructive value? The rest of Capella's work is so unreliable that one would not tend to regard him as a valuable source. Is there any reason for thinking that he preserves a particularly reliable tradition in the aspects which have been interpreted as relating to evidence for an Etruscan religion?

Piganiol: Martianus Capella is in complete agreement with what Seneca says in the *Quaestiones Naturales*, when he speaks of the Etruscans, which convinces us that he is right. I have compared a text by Martianus with one by Diodoros, in the days of Augustus, which sets forth the beliefs of the Chaldeans. Diodoros describes the Chaldeans of his own time, and it is extraordinary that Capella should attribute to the Etruscans exactly the same cosmogony as Diodoros, in the days of Augustus, attributes to the Chaldeans. We could say that the Etruscans may have taken their beliefs from the Chaldeans, or were influenced by them. What we call the Etruscan religion is actually the religion of the Chaldeans, who were travelling through Italy at the time of Cato. Prof. Banti has told me that Cumont observed that changes took place in the Etruscan doctrine in its final phase. This may be true, but we cannot be sure that it is true. Cumont, in trying to find the Etruscan doctrine, may have found some traces of Oriental doctrine. He considered it possible that Oriental influence had shaped Etruscan beliefs. Martianus Capella is very important, because the doctrine that he attributes to the Etruscans is the same as that of the Chaldeans in the days of Augustus, which is a truly extraordinary fact.

Bolelli: The problem of the origins of a nation is always very complex. In order to discuss the origins of the Etruscans, we should first

discuss, for example, the problem of Rome; and we would come up against some fantastically difficult problems. But it should be borne in mind that a considerable contribution, if only a negative one, can be made by language. Linguistics have already given a negative indication, i.e. they have shown what a language is not. It is easier to say what a language is not, than what a language is, but it is also important to know. Not only the Etruscan question is complex; it would be extremely difficult, even with the help of our archaeological colleagues, to find the origins of the Umbrians or the Oscans, without linguistic documents.

Strong: Are the skeletal finds from early excavations, particularly from Bronze and Iron Age sites, still available, or have they all disappeared by now?

Foti: Villanovan bones have been preserved in the past, but they may not be available for use as they came from cremations and only fragments of material may still remain. We can only make use of the skeletal remains from inhumations.

Bloch: Sergi studied this problem, and published his findings, *circa* 1920.

Ciotti: Some skeletal material may still exist, but may be quite mixed up and of no use.

Cook: It is unlikely that all the material has been lost, unless the Italian museums differ greatly from northern European ones, because there is a tendency for things to remain in a museum once they enter it.

Hencken: Skeletons are hard to find, but cremated bones exist in great quantity.

Oakley: As far as blood group determination is concerned, cremated bones are useless. As regards the size of a population, the ashes are weighable and quantities might be worth assessing from that point of view.

Cook: The biologists must not be discouraged, because the discussion has obscured the fact that many Etruscan tombs do, in fact, contain uncremated bones.

Piganiol: It seems to me that the archaeologists do not have a proper respect for skeletons; this applies to me too! In Africa, when we excavated a site and found a number of bones we lost many of them. But they are valuable, and now our scientific colleagues are warning

us to respect them more. Bones can be even more important than pottery. We should pass a resolution here that in future all skeletal remains should be treated with great respect.

Ciotti: It is not that we are wasteful or lacking in respect where bones are concerned. It just so happens that we have not been looking for them; they did not seem to be important and were lost with the other débris. But in future excavations all bones found should be classified and preserved so that they may serve as material for study for our other colleagues.

In the budgets for excavations an item should be included concerning the collection and preservation of bones. One of the reasons why bones have not been preserved is that this is too costly.

Riis: It is my personal experience that, in many museums, urns contain cremated bones but this is not always noted in the inventory. It would not be difficult to ascertain how much of this material exists. If, for instance, this symposium would lead to a request being sent to the museums which have Etruscan material, for information on how many urns contain cremated bones, I think we would get quite valuable material.

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HISTORICAL METHOD AND THE ETRUSCAN PROBLEM

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The problem of Etruscan origins is in no small degree an artificial one, the result of past confusions of method rather than of complexities inherent in the subject-matter. It has, for example, very often been assumed, explicitly or implicitly, that the origins of the historical Etruscan people and of the language that they spoke are necessarily one and the same. It is perfectly possible that they are. But there is nothing inherently impossible in the supposition that the strain within the Etruscan people that was responsible for the Etruscan language may have been quite distinct from the strains that were culturally and ethnically dominant. The same distinction applies with equal force to the concepts of culture and race. There has in the past been a tendency for the archaeologist (who is concerned primarily with the remains of the material cultures of the past) and the geneticist to assume that the patterns revealed by their researches ought in some way to coincide. In fact, it is becoming increasingly clear that the relationship between the two is very rarely one of simple coincidence. In other words, the origins of the Etruscan language, the sources of the historical Etruscan culture and the genetical origin of the historical Etruscan people are three quite distinct problems, each of which has to be tackled independently on its merits. Only in the light of such independent conclusions can one evaluate the points of coincidence and divergence.

There is nothing novel in this statement of the methodological requirements of the Etruscan problem, although it is worth

restating, since the careless habits of mind and speech inherited from a past generation of scholarship are still strong enough to call for constant watchfulness. What seems to be less generally recognized by the archaeologist and others is that in normal historical usage, when we refer to the people of a particular nation, the classification implicit in our minds is not usually one of language, culture or racial origins, but one arising from the political structure of the world at the time about which we happen to be talking. By the Indians today we mean the inhabitants of modern India, who have no common language, no community of race and who are only now beginning to acquire a community of material culture. Sometimes, of course, these various aspects may coincide very closely, e.g. in the case of the Arabs at the time of Arab invasions. Far more often, however, the coincidence is very far from complete. The political classification has no more inherent merit or scientific validity than any other. But it is the one that has the sanction of normal historical and everyday usage; and, being one that leaves very little tangible material trace of itself outside the written record, it is one that archaeologists are very apt to forget.

To the historian, then, the natural meaning of the question "Who were the Etruscans?" is "What was the genesis of the political entity which we find established in Etruria at the beginning of the historical period—namely the Etruscan League?" This is quite a different question from those asked by the archaeologist, the philologist, or the geneticist, whose interpretations of the same question may be paraphrased, respectively, as "What were the formative origins of the historical Etruscan culture?", "Whence did the Etruscan language derive?" and "What ethnic strain or strains can be traced in the historical Etruscan people?" All these are valid questions; but the essential prerequisite of any useful discussion is that they should be clearly and decisively distinguished. Failure to do so has been responsible for a great deal of the past muddle over the question of Etruscan origins.

These distinctions ought to be (but frequently are not) recognized also in our terminology. The term "Etruscan" should be applied with the very greatest caution to any aspect of human activity prior to the emergence of the Etruscan people into the light of history in the 7th Century B.C. Within the culture or ethnic make-up of the historical Etruscan people it may well be possible to distinguish certain dominant strains inherited from an earlier phase. But it is inviting confusion to refer to them at this earlier stage as "Etruscan"; "Proto-Etruscan" perhaps, provided that in each case one is very careful to define one's terms; but not "Etruscan". In the case of language or of political events there might be more justification in carrying the terminology of the historical period back into late prehistory; but even here it is probably wiser at this stage to choose a terminology that does not risk confusion with the results obtained by other disciplines.

In conclusion, one practical suggestion, arising out of the British School at Rome's current programme of field survey in southern Etruria. One of the most striking and interesting results of this work is the emphasis that it is throwing on the geographical independence and isolation of the individual city-states that made up the political entity that we know as the Etruscan League or Federation. These city-states have a strong individuality of their own; and it is becoming increasingly clear that in many cases they are the successors of archaeologically and geographically recognizable units that go well back into prehistoric times, long before the emergence of the historical Etruscan civilization. In other words, one must be very careful to avoid talking of the inhabitants of Etruria in the late prehistoric phase, as if they were a people uniformly distributed over the area or necessarily possessing a uniform culture or racial background. They were certainly scattered in what we may perhaps refer to as tribal units, in many cases widely separated by mountains and primeval forest-land; and these tribal units may prove, on closer examination, to cover a considerable range of cultural and racial

diversity. In our present very imperfect state of knowledge of the prehistory of central Italy, it would be unwise to say more. But the point is one that needs to be borne carefully in mind in any assessment both of the racial make-up of the territory in prehistoric times and of the relation of the individual Etruscan city-states to their prehistoric predecessors.

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MOVEMENTS OF POPULATIONS IN ETRURIA SINCE THE BEGINNING OF THE ROMAN REPUBLIC

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POPULATIONS are never static, except occasionally to the demographer. Men are always on the move to better their lot or out of spiritual restlessness, sometimes individually, sometimes in the mass: and when written or material remains seem to reflect such movements and its consequences, this becomes a proper subject of inquiry by the historian, although he can only rarely satisfactorily account for the causes. In this paper I am, of course, only concerned with the large-scale movements which at various times before the 7th Century A.D. may have changed the population of ancient Etruria.

The capture of Veii in B.C. 396,(1) only a few years after the last Etruscans had been expelled from Latium, provided Rome with its first permanent settlement in southern Etruria; the entire territory of the defeated city was redistributed among independent peasant farmers of Roman origin. In the succeeding century and three-quarters, the steady extension of Rome's authority in southern Etruria was associated with the establishment of colonies at Sutrium, Nepete, Cosa and elsewhere (Fig. 1). Some of the early colonies are known to have involved the settlement of only three hundred Roman families, each receiving not more than seven *iugera* (about four acres) of land. Others were no doubt

* The first part of this paper owes much to the suggestions and criticisms of Mr. A. N. Sherwin-White: but for misinterpretations of the Roman evidence and for any other errors I am wholly responsible.

considerably larger, perhaps already involving the settlement of two thousand and more families as in the colonies established after the Punic Wars.[1 bis] The total amount of early Roman

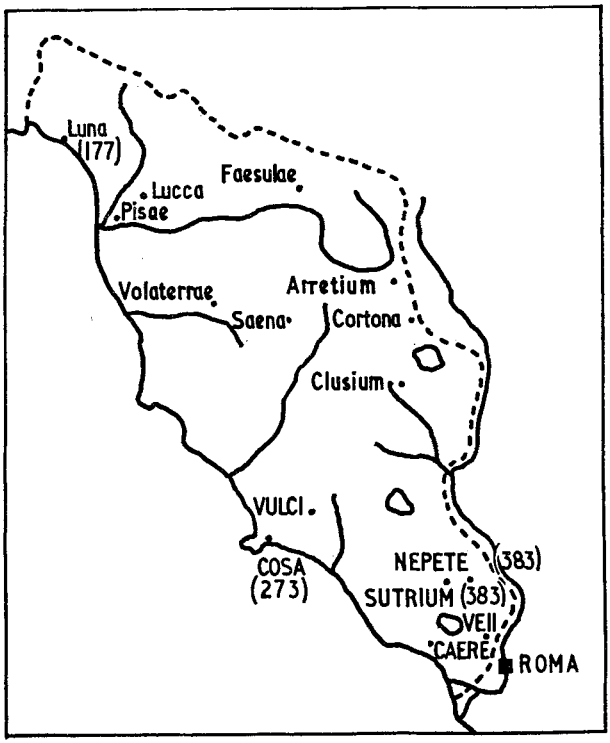

FIG. 1. Roman settlement in Etruria.

Towns colonized or otherwise subjected to settlement in the period *circa* 400–250 B.C. are indicated as COSA. Other towns are indicated as Saena. Dates are those of the foundation of colonies.

colonization in Etruria, however, could not have substantially affected the character of the population in more than a few relatively restricted areas.

Perhaps in the very years in which Rome began successfully to

extend its authority across the lower Tiber, armies of barbarians were crossing the Alps from the north.[2] These were the Celts or Gauls whose name for many decades spelt terror to the peoples of the northern Mediterranean, much as did their Germanic successors eight hundred years later. They brought with them a distinctive archaeological or artistic culture known, from the Swiss site where it was first identified, as "La Tène". The chronology of La Tène finds in Italy is still a matter of dispute among scholars.[2 *bis*] It is fortunate, therefore, that in a famous digression the historian Polybius gives a lucid and succinct account of Gallic expansion in Italy which depends on substantially reliable traditions,[3] and that his account can to some extent be amplified from the writings of Diodorus, drawing on an apparently independent tradition, and from other late historians. In the years immediately before and after 400 B.C., therefore, the Gauls steadily extended their hold on the Po valley and the Emilian plain. In or about the year 391 an army of Gauls crossed the Tuscan Apennines for the first time and appeared before Clusium. Clusium resisted successfully; but Rome, against which the Gauls then turned, was forced to sue for peace, one of the most humiliating episodes in her history. The treaty settlement between besiegers and besieged proved, however, to be the prelude to a complete withdrawal by the Gaulish hosts. This was perhaps only the most dramatic of a number of equally destructive raids and sieges launched by the Gauls into central Italy before the year 331. The Gauls made one more major incursion across the Apennines after this date, when they besieged Arretium in 285 or 284 and defeated a Roman army in the field. Shortly after this it was the Romans who were on the offensive against the Gauls.

That any significant number of Gauls settled in Etruria as a result of these expeditions is certainly to be discounted. Gallic cemeteries and Gallic place-names have been identified in considerable numbers north of the Po and in Piedmont. They are hardly less frequent in central and eastern Emilia. Except for a

single example from Canossa di Puglia, which may be connected with a known expedition in 367 or 349, the most southerly Gallic tombs so far uncovered are in the region of Osimo and Filottrano; this is, of course, a little way beyond the southern limits of Gallic occupation in this region (the so-called *ager Gallicus*) as specified by Livy, but this may be taken as a substantial confirmation, rather than the reverse, of the accuracy of his account.[4] In Etruria, on the other hand, no Gallic tombs or place-names have been identified by any competent scholar[4 bis]. The only known Celtic monument in this region is a bilingual funeral inscription from Todi,[5] which can hardly be used as evidence of settlement.

Central Italy was to suffer from one more barbarian invasion during the Republican period, that of the Carthaginians. More destructive to an ordered economic life than its immediate predecessors, no more than these did it introduce a new, alien, element into the population of Etruria.

If such an element was introduced at any time before the end of the Roman Republic it was not brought about by an invading army or by a mass movement of population establishing itself by force of conquest. Etruria may traditionally have been a land in which vast estates cultivated by tenant- and slave-labour existed side by side with the small estates of free peasant cultivators.[6] In the aftermath of the Punic Wars, partly as a result of the policy pursued by Rome towards the *ager publicus* seized from the towns which had failed to support her against Carthage, partly no doubt as a result of the social and economic disturbance caused by the Wars and of other, more complex, social and economic factors, estates of the first type came increasingly to predominate in many areas of central and southern Italy. A famous passage in which Appian gives a summary account of Roman 2nd-Century colonization and recolonization concludes with these words: "the rich, getting possession of the greater part of the undistributed lands confiscated from other towns by Rome and being em-

boldened by the lapse of time to believe that they would never be dispossessed, absorbing any adjacent strips and their poor neighbours' allotments, partly by purchase under persuasion and partly by force, came to cultivate vast tracts instead of single estates, using slaves as labourers and herdsmen . . .".[7] It is with the resulting slave-cultivated *latifundium* that Cato's treatise *de agri cultura:* written *circa* 175–150 B.C., is largely concerned. Neither Appian nor Cato was concerned particularly with Etruria; both, indeed, may have based their remarks primarily on the situation south of Rome. Etruria, however, is specifically named in a no less famous passage in Plutarch's *Life* of Tiberius Gracchus, quoting a pamphlet by Gaius Gracchus which explains how his brother came to see the need for land reform: "as Tiberius was passing through Etruria on his way to Numantia [he] observed the dearth of inhabitants in the country and that those who tilled its soil or tended its flocks there were imported barbarian slaves".[8] Three-quarters of a century later men like Domitius Ahenobarbus and Pompey could assemble substantial armies from the freed slaves and tenants of their estates in Etruria and its neighbouring regions.[8 *bis*]

Did the extension of or change-over to an essentially "capitalist" agricultural economy affect the population-pattern of Etruria as much as these passages suggest that it did? And is there any evidence as to where these "imported barbarian slaves" mostly came from? An answer to the second question, at least in terms of general probability, is more easily given than one to the first. A number of passages in Livy relating to the first half of the 2nd Century B.C. show that at that time Spain, Sardinia and Epirus were the main suppliers of slaves for the Italian market.[9] In the second half of the century continuing demand was increasingly met from eastern Mediterranean sources and the free port of Delos emerged as the great slave-trade entrepôt; Strabo speaks of the "numberless multitudes of slaves" which could be disposed of there in one day.[10] In 135 B.C. 60,000 slaves of Syrian

and Cilician origin are said to have been involved in a revolt in Sicily.[11] Marius' wars with the Cimbri and later campaigns across the Alps brought a substantial number of Germanic barbarians into the slave population of Italy, where they were joined in the 1st Century B.C. by African Berbers; both these groups were generally regarded as fit only for unskilled work on great estates. Asia Minor none the less continued to be the region from which Italy derived the greatest number of its slaves, particularly during the wars against Mithradates (90–65 B.C.) in Bithynia, Galatia and Pontus; during one of the earlier campaigns indeed the price of slaves in Lucullus' camp (the ordinary soldiers disposing of their share of the captives to richer men who could transport them to their estates) fell to four drachmae,[12] only four times the normal day wage of a free labourer of the period.

That "Orientals" and men of "Oriental" descent became an important element in the population of Italy in the Early Empire has long been recognized.[13] It may have been considered good economics in Cato's time to replace slaves by new purchases rather than by permitting them to reproduce themselves; but a century later than Cato, if not already earlier, slave-families were the exception rather than the rule.[14] It was not only the proletariat part of the population that was affected by this development. From the later 1st Century B.C. the manumission of slaves was an increasingly common practice; and freedmen and their descendants became a prominent feature of Italian political and economic life at the expense of the ancient free-born.[15] Men of this class are conspicuous among the landowners recorded in the "Table of Veleia" which is concerned with property adjacent to the northern Apennine boundary of Etruria.[16] Analysis of inscriptions relating to freedmen in Rome, Latium and Campania seems to show that at least two-thirds of them were of eastern Mediterranean origin.[17] No amount of ingenious guessing or dubious calculation, however, will tell us what this signifies in the proportion of total population.[17 *bis*] Still less will it tell us that was the effect of

these human "Oriental imports" specifically on Etruria; such evidence as there is from this region supposedly relates rather to free-born families who came to Italy from the East among the *clientela* of men like Pompey. Paradoxically, Rostovtzeff has contended that the "Orientalization" of the politically and economically predominant class was "slower in Northern and Central Italy, especially in the agricultural regions" than elsewhere.[18] This may mean that he supposes that the old families managed to maintain their slave- (or freed-slave-) cultivated estates longer in these regions than elsewhere, which would be interesting if true; but this is to deduce a hypothesis from a hypothesis. Unsatisfactory though it may be to leave the question like this, it is impossible to say more than that from the middle of the 2nd Century B.C. "imported barbarian slaves" of Oriental origin and their descendants cannot be left out of account in any discussion of the population of Etruria. But in the light of the evidence available to us we must conclude that, even if the rural economy of Etruria continued to depend for a long period on organized slave- or tenant-communities predominantly of eastern Mediterranean extraction, this could hardly have produced a significant differentiation of the later population of Etruria from that of other regions of central and southern Italy.

The end of the *pax Romana*, the efforts of land-hungry peoples of Germanic and Asiatic origin to expand or transplant themselves westwards into Europe, opened Italy once again to population movements of the more familiar kind. The first and most dramatic Germanic assault on Italy was that of the Visigoths under Alaric in the early 5th Century. Alaric may have intended at one time to establish some kind of permanent settlement in the Alpine regions. But no such purpose can be discerned in his advance to Rome: and the invasion touched Etruria only along the Via Flaminia or the Via Ameria.[19] When Rome fell in 410, Alaric went south to Bruttium, where he died, and his followers rapidly retreated northwards. Small groups of Visigoths may

have detached themselves from the expedition; but if they did they left no recognizable trace. Etruria was similarly largely or wholly unaffected by the 4th and early 5th Century "federate" settlements of barbarian warriors which were intended to protect Italy from this and similar invasions. The best-documented are those of the Asiatic (but Indo-European) *Sarmatae*.[20] The references in the historians and the Law Codes do not connect them with any particular locality; but place-names supposedly derived from the name of the *Sarmatae* (and several of them must be regarded as doubtful to say the least) are, as we would expect, located almost exclusively in the north. Tuscany can show only a very dubious example, an unidentified *Salmata* named in a badly-preserved document of the 9th Century.[21] A settlement of *Alamanni* in the Po valley is referred to by Ammianus.[22] The place-name and other evidence which might seem to indicate Almannic settlement elsewhere in Italy have unfortunately largely to be discounted; *Alamanni*, individually or in small groups, continued to enter Italy in the Lombard and Frankish periods: and many of the *Alamanni* place-names are probably connected rather with Lombard *arimanni(a)*.[23]

The mixed barbarian army under Odoacer that revolted in 476 because of the refusal of its demand for the *tertiae* of landed estates which was customarily granted to confederates was similarly stationed in the north, notably in the Milan region; and when after the success of the revolt, individual soldiers were allotted the *tertiae* they had demanded,[24] it is unlikely that any of them received land very far from the Alps and the Po valley. The next allocation of "thirds" of Roman-owned estates to barbarians, on the other hand, that which took place after the final triumph of the Germanic Ostrogoths under Theodoric in 493, did affect Etruria. Among the letters and commands dispatched from the court of Theodoric and his successor to all parts of Italy is one addressed to a Gothic *saio* (an administrative official) concerning "some of the Goths in *Picenum* and the

two Tuscanies [25] [who] are evading payment of their proper taxes".[25 bis] The Goths of *Picenum* and *Samnium* figure in other letters: not, however, the Goths of Tuscany, the ancient Etruria. Fortunately, the evidence of the royal letters can be amplified from contemporary accounts of the Byzantine conquest of Ostrogothic Italy. Among the towns named by Procopius as housing Gothic garrisons which were overcome in the campaign of 538 are Chiusi and Orvieto, each with a thousand Gothic troops.[26] The chronicler Agathias names among the principal centres of Gothic resistance in the later campaigns the cities of Lucca, Luni, Pisa, Florence and Volterra.[27] The last four of these did not, in fact, put up any serious resistance to the Byzantine army although the indication given by Agathias is none the less valuable. Lucca, however, was one of the last towns in Gothic hands to fall to Narses, at the end of 552. The Ostrogothic host that had accompanied Theodoric to Italy could not have been very large in comparison with the native population, certainly not more than 200,000 persons all told and perhaps substantially less, of which less than a quarter are likely to have been men capable of bearing arms.[27 bis] Accordingly the invaders were deliberately established up and down the country in fairly tight-knit groups which could easily answer a summons to arms: and measures were taken to prevent their losing their separate identity—they had their own officials and their own courts of law and were forbidden *conubium* with persons of Roman descent.[28] The literary sources show that Etruria and in particular its northern half now known as *Tuscia annonaria* received a number of such settlements.

Procopius' account of the campaigns of Belisarius does none the less suggest that the greatest concentration of Gothic settlement is to be looked for north of the Po; and this inherently probable inference is abundantly borne out by linguistic and place-name evidence.[29] There are some twenty names on the modern map of Italy which appear to go back to a primitive *Gothicum*, *Gothica* or simply *Gothi* and which may be supposed to reflect

Gothic group-settlement. Only one of these comes from south of the Apennines, specifically from the Benevento area, and that is of very doubtful antiquity. All but four of the others (three of them close to Ravenna where Theodoric built a palace) are in modern Lombardy, Veneto or Venezia Giulia. Professor Ernst Gamillscheg has identified something over ninety further names which in his view probably or certainly incorporate a Gothic personal name or other element. Many of these may well belong to the period *after* the destruction of Ostrogothic power in Italy; but in any case some sixty-five of them are in modern Piedmont and regions north of the Po. In Etruria there seem to be no more than eight such names: one near Lucca, three near Florence, three south of Arezzo and one near Grosseto. Gothic loan-words in later Italian and Italian dialects follow a similar pattern. More than half have been recorded only in the north; very many more are not found south of Benevento, and two or three are peculiar to Tuscany.[30] Finally we may note that archaeological finds, principally of brooches, which have been identified with a greater or lesser degree of justification as Ostrogothic, show a similar distribution; they occur all over Italy but they come predominantly from the north and in particular from the districts of Brescia (Lombardy) and Belluno (Veneto).[31]

The Byzantine military units left behind in Italy after the reconquest had been completed seem rapidly to have become dispersed communities of soldier-peasants associated with a castle (*castrum*) which was presumably occupied only in an emergency. Probably none of the Byzantine castle-territories created in what was formerly Etruria belong to the period of undisputed Byzantine rule [31 *bis*] and of the racial origins of those who formed the "garrisons" nothing is known. The garrisons of the older-established castles on the Alpine frontier failed dismally to prevent the most momentous of the Germanic invasions of Italy, that of the Lombards, who entered Italy from the north-east in 568.[32] Although the Lombard "host" may have been smaller even than

that of the Ostrogoths, warrior-bands were fighting their way down the Via Flaminia three years later. Unlike Theodoric, however, the kings of the Lombards never established their authority over more than northern and parts of central Italy; by the year 600 the Lombard-Byzantine frontier in the south was along a line which left the Perugia area and those regions of southern Etruria south of the Mignone and Lake Vico and east and south-east of Lake Bolsena to the Byzantines, although some of the Byzantine frontier *castra* had earlier been for a time in Lombard hands, and Orvieto was captured in 605 [33] (Figs. 2, 3).

Paul the Deacon's *Historia Langobardorum* and other literary sources tell the story of the Lombard military occupation of their kingdom in fair detail; but only occasionally do they give any indication of the manner in which the armies settled on the land after the battles had been won, and these passages are among the most debated in all the sources of Dark Age history. In the light of the evidence for the settlement of the Lombards' Ostrogothic predecessors, however, it seems unlikely that we shall go very far wrong if we start with the evidence of archaeology and place-names. The identification of brooches and other objects as Lombard is fortunately more than usually certain.[34] Only one of the recorded find-spots lies to the south of Rome and the next most southerly are two finds right on the old frontier near Sutri and near Bolsena, respectively. The other Lombard finds in Etruria are in fairly well-defined groups: one, including an extensive cemetery, round Lucca, a second to the south-east of Florence, a third near Perugia, and a fourth round Chiusi. The total number of find-spots and objects in Etruria is, however, small compared with those in Emilia, where they occur fairly thickly on either side of the Via Emilia east of Parma: and as with the Ostrogoths the greatest number of finds has been made in the north and north-east.

A basically similar pattern can be observed in the distribution of place-names which can be attributed with some confidence to

the Lombards.[34 bis] Of particular interest are those which derive from the word *fâra*. The most authoritative modern view is that the Lombard *fâra* was a unit of the host, either that which was placed under the command of a duke or a smaller part of it.

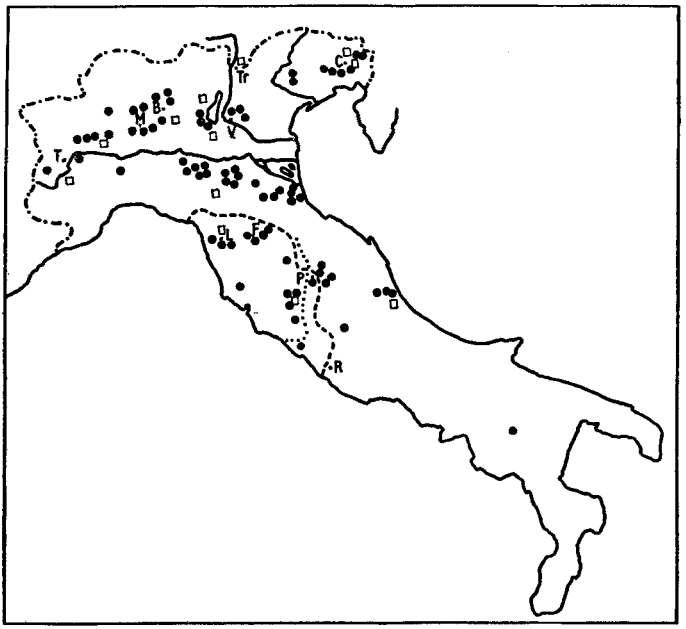

FIG. 2. Archaeological finds certainly or probably of Lombard origin.
□ = complete grave or cemetery; ● = individual find;
---- = boundary of Etruria;
.... = southern frontier of Lombard kingdom *circa* 600 B.C.
T = Turin; M = Milan; B = Bergamo; Tr = Trento;
V = Verona; C = Cividale; L = Lucca; F = Florence;
P = Perugia; R = Rome.

When it occurs as a place-name, it indicates a community of Lombard warriors of the kind with which the Ostrogothic settlement has made us familiar.[35] Nearly all the *fâra*-names are found in three areas: around Belluno, immediately to the east of Milan, and in those parts of Umbria and Abruzze which were

in the detached Lombard duchy of Spoleto. Such names are completely lacking in Etruria. By far the greater number of other place-names incorporating a personal name or other element probably or certainly of Lombard origin is found north of the

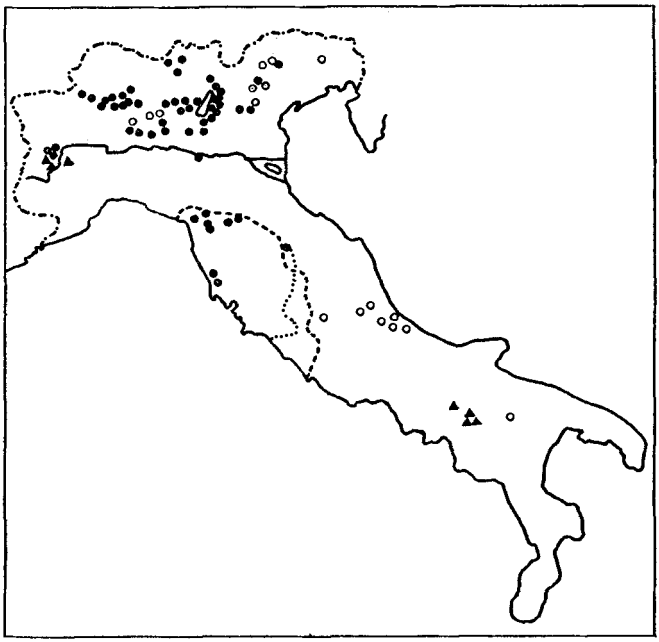

FIG. 3. Place-names supposedly of Lombard origin.
▲ = *Langobardi*-names (Gamillscheg II, p. 69 ff.);
○ = *fâra*-names;
● = other names incorporating a Lombardic element;
---- = boundary of Etruria;
.... = southern frontier of Lombard kingdom *circa* 600 B.C.

Po, in particular in Lombardy and immediately to the east of Lake Garda (Fig. 3). The few that there are in Etruria occur entirely in its northern half, in other words in the area in which most of the archaeological finds have been made.

The concentration of archaeological remains and place-names

in relatively few areas seems to accord very well with recent interpretations of other evidence for the manner in which the Lombard warriors, the *arimanni*, lat. *exercitales*, settled in the regions of Italy which they conquered. They originally established themselves in groups in easily defendable positions on the outskirts of the old *civitates* or in similar "castles" in the countryside; these communities probably lost their distinctive racial character fairly rapidly after the conversion of the Lombards from Arianism to the Catholic Christianity of their Italian neighbours in the early 7th Century, but many of them are traceable in later references to *castra* or to *arimanni(ae)*.[36] The remarkable studies of Professor Bognetti of Milan have suggested that other kinds of evidence, such as church-dedications, may also point to one-time communities of Lombards; and by the application of his techniques another scholar has made out a convincing case for substantial Lombard settlement on the Etrurian side of the upper Tiber, for which there is neither place-name nor archaeological evidence.[37] It is unlikely, however, that the extension of such methods of inquiry to other areas will seriously affect the general picture of Lombardic settlement: one in which the north is seen to have been settled much more intensively than the lands south of the Apennines.

For more than a thousand years after this, Italy was subjected to invasion and despoilment at the hands of the armies of what many of the inhabitants of the peninsula were (and are) inclined to regard as uncivilized barbarians; but none of them was of a kind that introduced any numerically significant new element into the population of central Italy. Population movements came to an end here with the migration of the Lombards; and it is time to summarize the conclusions of this contribution to the present discussion. Of the invaders of Italy in the thousand years that extends from the beginning of the 5th Century B.C. to the end of the 6th Century A.D. only the Germanic Ostrogoths and Lombards, at the end of this period, established real settlements

in what had anciently been Etruria. Planted in groups, originally more or less selfcontained, in a limited number of places, the total numbers involved were manifestly small both in comparison with the native population and with comparable settlements north of the Apennines and in particular north of the Po. No invasion or mass migration seems likely to have affected the population of post-Etruscan Etruria as much as the partial depopulation of its agricultural areas in the 2nd and 1st Centuries B.C. and the introduction in large numbers of slaves of western Mediterranean, Transalpine but predominantly eastern Mediterranean origin; if one of the consequences of this development was an "Orientalization" of the population, or at least of the rural population, of Etruria, this is a phenomenon which that region shared with other areas of central and southern Italy: and to the question; "What did this mean in terms of absolute numbers or even percentages of the total population?" the honest historian must admit that he has no answer.

NOTES

1. G. de Sanctis, *Storia dei Romani*, II, Turin-Florence, chs. 15, 17 *et seq.*; C[*ambridge*] *A*[*ncient*] *H*[*istory*] VII, 1928, chs. 15, 18.

1 *bis*. The 300-family colonies were those founded on the coast, the first *coloniae Romanae*, the number of families involved in the (inland) *coloniae Latinae* is nowhere stated. It is not clear why the excavator of Cosa, Professor F. Brown, supposes that it was founded with "2,500 or 4,000 families", a figure which seems much too high for its restricted territory.

2. De Sanctis, ch. 16; *CAH*. VII, chs. 2 (de Navarro), 17 (Homo), F. W. Walbank, *A Historical Commentary on Polybius*, I, Oxford, 1957, p. 182 ff.

2 *bis*. Compare Navarro *cit.*, p. 62. The position has not been substantially clarified in the last thirty years.

3. Polybius II, 18 : based on Fabius Pictor, Walbank, p. 185, cf. p. 27, *contra* Homo *cit.*

4. Livy V, 35, 2–3. For the finds, see von Duhn in Ebert, *Reallexikon f. Vorgeschichte*, VI, Berlin, 1926, p. 291 f.

4 *bis*. Professor Riis drew my attention to the Gallic character of the armour shown on *stelae* from Castiglioncello (*Notizie degli Scavi*, 1924, p. 167 ff.). But the character of the rest of the find suggests that Gallic *influence* is a sufficient explanation.

5. G. Dottin, *La Langue Gauloise*, Paris, 1920, p. 153 n. 17 *bis*.

6. So M. Rostovtzeff, *The Social and Economic History of the Roman Empire*, ed. 2, Oxford, 1957, p. 23.

7. Appian, *Bellum Civile* I, 7 (the translation is Tenney Frank's).

8. Plutarch, *Ti. Gracchus* 8, 7.

8 *bis*. Rostovtzeff, *Roman Empire*, pp. 30, 549 n. 19, rejecting the excessive doubts of W. Heitland in *Journal of Roman Studies* VIII, 1918, p. 38.

9. Livy XXXIV, 16, 21; XXXVIII, 29; XLI, 28, etc.

10. Strabo XIV, 5. 2. For an early example of slave-trading at Delos see the inscription in *Bulletin de Correspondance Hellenique*, XXXVIII, 1914, p. 250 ff. (Vallois.)

11. Appian, *Bell. Civ.* I, 9.

12. Plutarch, *Lucullus* 14, 25.

13. See especially T. Frank, "Race Mixture in the Roman Empire", *American Historical Review*, XXI, 1915/16, p. 689 ff., and the other works listed by Rostovtzeff, *Roman Empire*, p. 583, n. 33.

14. Appian, *Bell. Civ.* I, 7; Varro, *Re Rustica* I, 17, 5.

15. Rostovtzeff, *Roman Empire*, pp. 32 f., 562–3, n. 18, 583 n. 33. Cf. *Tactius*, Annales XIII, 27: "Most of the knights and many of the senators were descendants of slaves".

16. *Corpus Inscriptionum Latinarum*, XI, n. 1147, with the observations of F. G. de Pachtere, *La Table hypothécaire de Veleia*, Paris, 1920, esp. pp. 87 and 95, and of G. E. F. Chilver, *Cisalpine Gaul*, Oxford, 1941, p. 155 ff.

17. M. Bang, "Die Herkunft der Röm. Sklaven", *Römische Mitteilungen*, XXV, 1910, p. 223 ff., XXVII, 1912, p. 180 ff.; Tenney Frank, *art. cit.*

17 *bis*. Cf. Frank, *An Economic History of Rome*, ed. 2, London, 1927, p. 213, who reaches "the surprising conclusion that nearly 90 per cent of the population permanently resident at Rome in the Empire bore the taint of foreign extraction".

18. *Roman Empire*, p. 583, n. 33.

19. T. Hodgkin, *Italy and her Invaders*, ed. 2, I, Oxford, 1892, p. 702 ff.; E. Stein, *Geschichte des spätrömischen Reiches*, I, Vienna, 1928, p. 389 ff.

20. T. Mommsen, *Gesammelte Schriften*, VI, Berlin, 1910, p. 257 ff. The principal references are also assembled in F. Schneider, *Die Entstehung v. Burg u. Landgemeinden in Italien*, Berlin-Grünewald, 1924, p. 135, n. 1.

21. Schneider, p. 136, n. 2.

22. Ammianus XXVIII, 5, 15.

23. On which cf. below.

24. Procopius, *Bellum Gothicum* I, 1, 5–8.

25. Etruria and part of the former Augustan region of *Umbria* now formed the two provinces of *Tuscia annonaria* and *T. suburbicaria*.

25 *bis*. Cassiodorus, *Variae* IV, 14 of 507/11 (ed. Mommsen, Berlin, 1894, p. 121).

26. Procopius, *Bell. Goth.* II, 11. But admittedly his account shows the

Ostrogothic king placing the garrisons in those cities after the campaign had begun.

27. Agathias, *Historia* (ed. Niebuhr, Bonn, 1828) I, 11.

27 *bis*. The figure cautiously accepted by Hodgkin, *op. cit.*, III p. 182 f.

28. The classic discussion is that of Mommsen, *Ostgotische Studien* = *Ges. Schr.* VI, pp. 362–484. For a rather different view, based on several important recent studies, see G. Vismara, "Romani e Goti di fronte al diritto nel Regno Ostrogoto", *I Goti in Occidente*, Spoleto (Centro di Studi sull'Alto Medioevo) 1956, p. 409 ff.

29. What follows is based essentially on E. Gamillscheg, *Romania Germanica*, II, Berlin-Leipzig, 1935, p. 1 ff.: many of his etymologies and other details are questionable but corrections of detail are unlikely to affect the broad outlines of his conclusions. For the lexical side in particular, see also C. Battisti, "L'elemento gotico nella toponomastica e nel lessico italiano", *I Goti in Occidente*, p. 621 ff., who however accepts Gamillscheg's etymologies too readily.

30. But several of Gamillscheg's examples were at one time more widely distributed.

31. The standard account of Ostrogothic finds in Italy is still that of N. Åberg, *Die Goten u. Langobarden in Italien*, Uppsala, 1923. But an important new work can shortly be expected from the pen of Professor Joachim Werner of Munich.

31 *bis*. For these castle-territories and their supposed Byzantine origin, see especially F. Schneider, *Die Reichsverwaltung in Toscana*, Rome, 1914, p. 36 ff. and ch. 3 *passim*, ib., *Burg u. Landgem.*, p. 4 ff. For a rather different account of a castle on the northern edge of ancient Etruria see *Papers of the British School at Rome*, XXIV, 1956, p. 14 ff.

32. Hodgkin, *op. cit.* V, ch. 4 *et seq.*; L. M. Hartmann, *Gesch. Italiens im Mittelalter*, II/1, Gotha, 1900.

33. A detailed account of this frontier in Schneider, *Reichsverwaltung*, p. 17 ff. The excellent map in Hodgkin V, facing p. 353, should be corrected accordingly.

34. Åberg, *Goten u. Langobarden*, p. 39 ff.; Deutsches Archäolog. Inst. (S. Fuchs, J. Werner), *Die Langobardischen Fibeln aus Italien*, Berlin, 1950.

34 *bis*. Gamillscheg *op. cit.* p. 59 ff.

35. The *fâra*-names were first collected and their significance discussed by Hartmann *op. cit.*, pp. 43 f., 52 f. The modern view is most fully stated by Bognetti in G. P. Bognetti *et al.*, *Sta. Maria di Castelseprio*, Milan, 1948, pp. 386 n. 37, 390 n. 39 and *passim* (indice, *s.v. fâra*).

36. Bognetti, *op. cit. passim*, with ample references to previous studies of which the most important since Schneider are his own.

37. G. Franceschini, "Chiese a coppie nei territori arimannici dell'alta valle del Tevere", *Atti del 1° Congresso internazionale di Studi Langobardi*, 1951, Spoleto, 1952, p. 323 ff.

GROUP DISCUSSION

Ward Perkins: Many of the points which Mr. Bullough has dealt with in general terms can and undoubtedly will be clarified in terms of more detailed regional work. The point that he has made about the big *latifundia* in late Republican Roman times is well substantiated in the literary sources; it is also one, if one turns to the historical parallel of the present day, that is always going to hit the press. But one has to remember that side by side with the *latifundia* there were undoubtedly in Etruria (and one can document them on the ground) large numbers of small settlements. There were not only the official *colonae* (though they are an important factor, particularly such a well documented one as Cosa), but there were large numbers of settlements that were allocations of property to the disbanded armies of Roman generals, particularly in the last century of the Republic. One of the clearest results that is emerging from our own work in the area immediately north of Rome is the extent of these settlements. In the area around Veii, for instance, or again in the area around the western part of the Ager Faliscus there are extensive settlements of the late Republican period, dated approximately 2nd–1st Century B.C. This is very closely comparable in density to the present-day land resettlement which is going on in Italy. From that point of view it is also very interesting that we have a very close parallel at the present moment, where the big *latifundia* of Etruria are being broken up and distributed to settlers who are being brought down from the more land-hungry parts of Italy, from the Marche and from the Abruzzi. One gets a pattern, in the surviving archaeological remains, which is quite definitely recognizable. These small farms, all of them datable approximately in terms of the late so-called Campanian black-glaze pottery which they used, all follow a very close pattern on the ground. One has to bear these in mind, and it emphasizes what Dr. Ciotti said earlier, that any evidence that we apply has to be applied very closely in terms of local regional knowledge. One will not be able to make broad generalizations. For instance, it is reasonably certain that whereas large areas of the Maremma may have been *latifundia*, equally large areas of central

Etruria were in the hands of the small-holders. Even that is a generalization that calls for local qualifications, but something of the sort must apply.

In regard to the slave population, which is a point relevant both to the *latifundia* and the small-holdings: Mr. Bullough has said that we can never make more than wild guesses. That is true; but it is well to know that we have, at any rate, some control figures for it. We do know that in Rome itself the middle and lower classes of middle Imperial Rome contained an astonishingly high proportion of people of ultimately slave origin and mostly from the eastern Mediterranean. The kind of figure which one gets from analysis of inscriptions, as for instance inscriptions from the recently excavated cemetery under St. Peter's, gives an ascertainable figure of over 50 per cent of people of ultimately slave and ultimately Oriental origin. It may well be that in the artisans and merchant class one is dealing with a figure of something like 75 per cent. That does not necessarily apply to the rural populations; but it does apply in a lesser degree to the urban centres within Etruria. It is probably reasonable to say that the dilution gets more and more as one gets out into the small country estates and into small villages where one is dealing with a more stable population. Therefore, any work that one does must be very closely related to the complete circumstances of the find. One must try to know whether one is dealing with the remains of a large or small estate, of an urban centre, or of a purely agricultural country village.

Strong: The repopulation of the small-holdings which took place in the last two centuries B.C. will not necessarily mean that the people who recolonized them were themselves Etruscans. The corollary of the depopulation of Etruria is the swelling of the Roman "plebs". Many Etruscans will have stayed and been permanently absorbed into the Roman population. The subsequent repopulation and reallotment of land therefore produces an ethnic change-over in that area. However much it is repopulated, the fact remains that the new settlers will probably not be Etruscans.

Ward Perkins: The analogy of today would suggest that in a great many cases they will not be Etruscans.

Strong: The evidence of extensive emptying of people from Etruria indicates a very important change in the population of certain areas.

The aristocracy, in particular, probably went where the opportunities existed.

Ward Perkins: That seems more questionable.

Strong: The number of supposedly Etruscan names amongst the aristocracy of Rome, particularly under the Roman Empire, when it was much more mixed than under the Republic ,is fairly high.

Ward Perkins: It is fairly high, but if you study the epigraphy of the Viterbese or of the smaller areas, you still find quite a number of people who maintain their own landed estates.

Kirk: From the discussion so far it emerges clearly that there were enough changes in the population of Etruria (changes which are not parallel with changes in any other parts of Italy) to explain the distinct appearance of Etruria in Dr. Mourant's blood group map (1954, The Distribution of the Human Blood Groups, p. 336. Oxford: Blackwell) in which there seems to be a significant difference between central Italy and the northern and southern halves.

Mourant: That does not appear quite so clearly now in a revised map of Europe (Mourant, A. E., Kopec, A. C., and Domaniewska-Sobczak, K. (1958). The ABO Blood Groups: Comprehensive Tables and Maps of World Distribution. Oxford: Blackwell). There is a fairly progressive change from north to south, but not an Etruscan enclave as appeared before.

Kirk: In any case, it would perhaps be dangerous to assume that by using the present population you could get any kind of valuable indication of the blood group distribution in ancient Etruria.

Ward Perkins: There are areas where you could attempt it; one area worth studying would be around Pitigliano, the upper Fiora Valley, where we have very little evidence of any Roman displacement; as far as I know there is no evidence of early mediaeval displacement in the area west of Lake Bolsena.

Bullough: This area is traditionally regarded as having been virtually uncultivated. There are no records.

Ward Perkins: There are the ancient sites; mediaeval towns are on top of the ancient sites. You can go a long way around Pitigliano, for instance, before you actually find the remains of a Roman villa. It may well be that there were some large Roman estates in the area, and there may have been some foreign slaves cultivating them. But all the

evidence suggests that the substantial basis of the population remained stable. The same is true in parts of the central Viterbese where there is a very strong Etruscan pattern, in the archaeological sense, a very strong mediaeval pattern, and very little Roman. It was off the main roads, and again, there may have been in terms of ownership of the large estates a certain infiltration of Oriental slaves, but that would have been only a very small percentage of the Etruscan population. I think that, if one chooses with care, there are areas where one can say that on historical grounds the likelihood is that there has not been a very substantial ethnic change.

Mourant: On general principles I would agree. When our first map was made we had rather little evidence from Italy, and some of the features which appeared in the map were just accidental, owing to such causes as the samples being too small. The present map which I have prepared probably gives the main pattern for Italy with most of these accidental things ironed out. I do not know how much evidence Dr. Morganti is going to produce in his paper, but in this country we are now reaching the stage of detailed mapping of blood groups where one can find small areas showing genuine differences from the surrounding areas, differences which may mean something in terms of population history.

Strong: Have not these places been backwaters throughout history, also at the time that the Etruscans appeared?

Ward Perkins: That is probably very true. This carries us back to the whole essence of this discussion. I think myself that, at Pitigliano, you could probably go right back to the late Bronze Age population. The big cities, on the other hand, or the coastal areas where there was a great deal of coming and going present a much more difficult problem. But we have some chance of controlling that particular aspect, by going to some of the sites which were certainly intensively occupied in Etruscan times and subsequently completely abandoned. We have every chance of getting good Etruscan material, without getting infiltration from Veii, for instance. That will take a lot of relating to adjoining areas, which are somewhat different.

Kirk: At Veii there has been no subsequent population change?

Ward Perkins: Veii is very tricky. It has the most intensely occupied stretch of small Roman settlements that we have yet found. The area immediately north of Veii has a farm every few hundred yards.

Kirk: What one wants for control purposes is a genuine Etruscan site with typical Etruscan cultural manifestations; an area as isolated as the area west of Lake Bolsena, which has not been touched by later population changes.

Ward Perkins: Saturnia is a good Etruscan site which has been very little touched. There is a certain amount of mediaeval settlement on it; I have never heard of anything odd going on there in the way of population movements. It is an isolated little place.

Barnicot: We have been speaking about population movements in mediaeval times and a little earlier. The first thing one should do is to see whether the population has been stable in the last 200 years; from church records one would get more definite information. If one could be sure that there has not been slow infiltration of people to and from other areas during such a limited period, it might be worth study. It is better to start in a period for which the most detailed record is available, and to make certain that the population is stable.

Ceppellini: Cavalli-Sforza is doing some research in the Apennines, using church records going back to 1600 A.D., which shows that in these mountain villages the population is very stable. In general, in my experience, it is certainly very important to study for guidance small villages, small populations, small genetical units. The present maps are of relatively little use, because the data are drawn mainly from the big towns where there is extensive mixing. It is amazing, as I shall show you later (this symposium, p. 177), how many variations there are from village to village. They differ as much as do different races.

Lehmann: Is that not a question of sample?

Ceppellini: No, we sampled all the villages.

Lehmann: If you sample a big town it shows a mélange of all the people; you get a random sample which gives you an idea of the whole population.

Ceppellini: We do not want to know the whole population: we made a topographic map for this reason.

Lehmann: If you go from village to village you are liable to get artifacts. If—say in the year 1405—there was an epidemic in a village and only a few couples survived who proceeded to repopulate it, then in 1905 you will find mostly the blood groups of these few individuals when you examine this village. Similarly if there has been at any

time a particularly potent and procreative gentleman, his blood groups will be unduly frequent in later years. You can avoid this "local accident", this genetical shift, by having samples not from a single village or two—however large—but many samples taken from as wide an area as possible.

Ceppellini: I think we must not avoid these small incidents. There are many factors which change gene frequencies, i.e. the factors of evolution and, as you say, those that may date from a very fertile gentleman, or from a given disease or from a whole settlement. But we must work for years sampling these small villages, the small population units, and see if some kind of pattern arises, which will fit with the pattern of the historians and archaeologists, etc.

Banti: We must beware of migrant settlements, because I am certain that Etruscans never went up in the mountains. They had very nice land in the plains, or at the foot of hills. I believe that the Apennines have remained under the Umbrians and Ligurians, those populations which were pushed out from more fertile areas. As far as I can see archaeologically, mediaeval northern Italy has not one Etruscan settlement and no Etruscan remains in the Apennines. They are to be found only in the plains and particularly in fertile valleys near those plains.

Ceppellini: If we want to know the Etruscans we must also know the other populations; therefore we cannot limit ourselves to the study of a few spots. We must try to have as complete and detailed a map of Italy as possible.

Banti: That is quite true; but beware of getting your results from the mountain populations. As has already been said, small villages in the mountains always have their own character and those are always the same, from year to year, from century to century. It may be that those populations, just because they are settled there for centuries, are not Etruscan, and have not been touched by Etruscan domination.

Ceppellini: We must only produce blood groups; and it would be up to you to give names.

Morganti: If the population has remained unchanged in certain mountain regions, this is most interesting for us, because the possibility of a blood group investigation is based mainly on the comparison of distribution in different places. If it is true that the mountain regions never came under the influence of the Etruscans, then we shall be able

to compare those with the neighbouring populations. If we cannot be sure of this, then we have no ground on which to base our comparative investigations.

Banti: It is an interesting fact that along the Arno we have Etruscan traces from the 2nd Century B.C., whereas in the mountain districts we find no such traces, unless we consider the sanctuaries. I am speaking now of the north-eastern frontiers of Etruria. As you follow the plains, you find traces of Etruscan remains, but there is a clear line of demarcation between the plains and the hills, and beyond this line no traces are found.

Cook: Which of the southern Etruscan sites are likely to have been least disturbed by infiltration?

Foti: The least amount of infiltration took place in inner Etruria, in the region of the rock tombs, i.e. in the region referred to by Mr. Ward Perkins, around Castel d'Asso and Barbarano.

Morganti: The archaeologists and historians should give us an idea of how the population areas should be grouped. So far, our criteria have been based on distinguishing between populations by means of regional boundaries; but if you can suggest other criteria corresponding more strictly to the ethnic divisions, our data can be much more informative.

Cook: Concerning the slave populations in Roman Republican Etruria: firstly, is there some particular type of terrain on which *latifundia* were most common? Secondly, how far were these agricultural slaves allowed to breed? It seems to me that, on the general evidence, the agricultural slaves were predominantly male and were kept more or less as farm workers without any intention of raising new stock.

Bullough: On the second question: Cato said that there was no point in keeping them alive. But there is evidence of even the agricultural slaves having families. It is quite clear from the evidence of some of the Campanian villas that there were slave families.

Ward Perkins: In answer to the first point, those are not really parallel. The big *latifundia*, of which I was thinking, parallel the big estates of the same area. One has every reason to imagine that the *latifundia* were based on a very similar economy. They were essentially stock-breeding. On the whole, there is a reasonable assumption that they covered predominantly the areas which are covered by the great stock-breeding estates that were found thereabouts up to the beginning

of the present century; that is to say predominantly the Maremma and the plains, like parts of the Viterbese, and not the areas where one gets the small villages, which obviously had an economy very much like the one which one sees there today. Perhaps there too one must make a distinction. We may confuse those who are not familiar with the area when we talk about "mountain villages", with respect to Etruria; in the sense in which Prof. Banti was referring to mountain villages, they are found in a relatively small marginal area in northern Etruria whereas there are a great many villages which have much the same characteristics, but are scattered over central Etruria, which is extremely hilly but not mountainous like the Apennines.

Banti: It should be re-emphasized that the Etruscans never settled in the Apennines, but they certainly settled in the hilly zone of Etruria. It is quite safe to look for them in those places which you mention; but not in the mountain zones where today you still have the descendants of the Bronze Age or the Neolithic populations.

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THE PRINCIPLE OF ETHNIC SUBSTRATUM. TRACES OF ETRUSCAN IN TUSCAN DIALECTS

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In 1881, in the first linguistic letter published in the *Rivista Fil. Istruz. Class.*, the great Italian linguist Ascoli enunciated the principle of ethnic substratum, a fundamental principle of research in the identification of particular phenomena observable in modern languages and their reference to languages formerly spoken in the same territory. This famous principle may be formulated thus. When a language is ousted by another, the victor language does not escape transformations due to the vanquished one. The latter does not disappear without leaving some vestiges behind it.

Accordingly, in the case of Latin, the various Romance languages presumably contain particular phenomena dating back to the languages spoken before the onslaught of Latin. Ascoli, indeed, had the case of the language of Rome clearly in his mind when he undertook to seek out Celtic elements in the various Romance languages. Latin is an outstanding example. From being the language of a small rural community in Latium, it underwent many changes in its gradual expansion, first among the bordering populations and then among peoples dwelling in regions far removed from its centre. Even so, it preserved a powerful unity thanks to its literary and cultural traditions.

The principle of ethnic substratum has often been discussed. It has been possible, for example, to speak of "substratomania" and "substratophobia", to use Rohlfs' terms, and at times facts

attributed in the first enthusiasm of research to the substratum have been liable to a different interpretation.

It is important, however, to come to a preliminary understanding as to the meaning of the qualification "ethnic", as this word may give rise to questionable interpretations. It would be a mistake to believe that differences in language must accompany ethnic differences, but it would be an even greater mistake to believe that a difference in language must go hand in hand with differences in the anatomy of the organs of speech. Vendryes' remark that a young Negro child, brought to Paris, will speak excellent French, absolutely indistinguishable from the French of the most authentic Parisian, is obvious. The objection has, however, been made that psychophysical differences must be accompanied by differences in language. This objection is easily answered, for every individual has his own particular language; but what we are concerned with here is not this individual language, but language as a social fact, as a means of communication among men.

We here venture to suggest that it would be necessary that the expression "organs of speech" should be replaced by the more strictly correct expression "habits of articulation". Clemente Merlo himself, who has done much excellent work on the substrata of Romance languages, if he sometimes uses the expression "organs of speech" (Merlo, 1927)* does so, as he states (Merlo, 1954), in the sense of habits of articulation.

Having thus placed the problem, not on a physiological, but on a linguistic basis, and having confirmed the independence of language from race, it is evident that when a people changes its language, it tends to introduce into the new language articulating propensities and habits proper to its former tongue. Phenomena pertaining to the substratum may be identified in some instances,

* See *Ital. Dial.* (1927), III, p. 4 of abstract: "These are, to my mind, very considerable phonetic alterations, the evident signs of organs of speech differing profoundly from Latin or Italic in general, a valid proof of their Etruscan origin."

though not in others. In the necessary circumspection, which is incumbent on such studies, specially favourable conditions must occur for identification to be possible. Such is the case of south-central Italy where, in a practically compact area, present-day dialects exhibit the phenomena *nd* > *nn* and *mb* > *mm* (*quanno* for "quando" *ritonno* for "rotondo", *gamma* for "gamba", *immocca* for "in bocca", etc.). Now this is the very area where we know that Latin was adopted by populations that spoke Oscan and Umbrian, languages which evinced these very phenomena, as can be fully proved by their ancient documents (e.g. *u*, as in *umen* < omben = Lat. unguen; o. *upsannam* = Lat. operandam). Evidently, when those populations learned Latin they introduced into Latin those phonetic elements that belonged to their former language.

As for the traces of Etruscan that have been left in Italian dialects, the problem is doubtlessly complicated, as Tagliavini (1951) pointed out, by the fact that the Etruscan territory underwent considerable changes in the course of centuries. From Etruria, properly so-called (corresponding more or less to what Tuscany is at the present time), the Etruscans spread north towards the valley of the Po and the Alps, and south towards Rome and beyond. Etruria, however, was without doubt the territory where the Etruscan population put up the greatest resistance both before and after the Roman conquest.

Now the various dialects that are spoken in modern Tuscany present characteristics that are nearer Latin than any that are spoken in other Romance territories. One of these Tuscan dialects, with a singularly eventful destiny, gave rise to the Italian literary language. This fidelity to Latin must be attributed to the slight influence of the linguistic substratum. This substratum was Etruscan, the characteristics of which are radically different from those of Latin and gave rise to no partial blending, but admitted only of a radical change of language, according to the well-known law that when two languages come together their mutual influence is directly proportional to their similarity.

But in all Tuscan dialects there is one important characteristic, the aspiration of the plosive, unvoiced, intervocalic consonant *-k-*. In an extensive area comprising Prato, Florence, San Miniato, Certaldo, Monteriggioni, Castelnuovo Berardenga to the west, Montevarchi, Reggello, Pontassieve to the east, and including also Figline and San Giovanni Valdarno, the aspiration extends also to intervocalic *-t-* and *-p-*, so that not only *la hasa*, *la honca*, but *staho*, *Praho*, *topho*, *lupho*, *scopha*, etc. are found. In the western group of Tuscan dialects the phenomenon is limited to *-k-*, but instead of being aspirated the consonant simply dissolves. One gets, for example, *vi-o-lo*, *amio*, *fio*. This fact has actually occasioned some mistaken reconstructions, as for instance in the words *bugica*, *camelica*, *ciprica*, *Vicareggio*, *pagare il fico*.

This phenomenon of aspiration has been attributed to Etruscan. The arguments in favour of the hypothesis have been given the most exhausting exposition by Clemente Merlo. In his first work on the subject, in 1927 (p. 84), Merlo mentioned Nissen (1883) as the first linguist who referred Tuscan aspirated consonants to Etruscan, followed, nearly contemporaneously, by Hugo Schuchardt (1884). This hypothesis, on the contrary, seemed hardly worth considering to that very Ascoli who was the initiator of the studies on the substratum (see Ascoli, 1887).

Other distinguished linguists, like Meillet and Bréal (1907), took a favourable view of the hypothesis of the Etruscan origin of the Tuscan *-h-*. Meillet summed up the question in the following way in "Esquisse d'une histoire de la langue latine" (1928): "En Toscane, les consonnes occlusives telle que *p*, *t*, *k*, et *b*, *d*, *g* ont une prononciation différente; on a, dès longtemps, mis ces faits en rapport avec des particularités de prononciation que révèle l'étude des textes étrusques et, récemment, M. Merlo a confirmé l'hypothèse en déterminant avec précision l'aire de ces faits toscans qui concorde presque exactement avec l'ancien domaine étrusque."

The acceptance of the theory, and in a book so widely known

as Meillet's "Esquisse", seemed to be its definite sanction. In the work of von Wartburg, "Die Ausgliederung der romanischen Sprachräume" (1950), the problems of the substrata are re-examined and aspiration is considered to be an Etruscan fact. In quite recent times, however, some disagreement has been expressed as to this view.

Merlo, even in his first paper quoted above, observed that Etruscan had aspirated labial, dental and velar consonants. The inference that Latin plosives were pronounced by the Etruscans (if not actually as real aspirated consonants, still as plosives followed by an aspiration) was natural. But even this apparently indisputable point was contested by some critics; first by Rohlfs (1930), then by Hall (1949) who takes up an absolutely sceptical position, and goes so far as to say: "The exact phonetic value of the Etruscan symbols is not known, nor is the nature of the phonetic contrast between the two series *p t k* and φθχ in Etruscan. In this case we must simply say *non liquet.*"

This attitude seems absolutely unwarrantable. If there is an ascertained fact in Etruscan phonetics it is just the trend of voiceless plosives to become aspirated. Pallottino (1936) summarizes this tendency as follows:

$$\begin{aligned} c &> \chi \quad (> h) \\ t &> \theta \quad (> h) \\ p &> \phi > f \ (> h) \end{aligned}$$

with the examples: *zac* (*a.* Etr.) > *zaχ* (*n.* Etr.); *caspri* > *haspri gent. atre-* (*a.* Etr.) > *aθre* (*n.* Etr.); *θui* > *hui* "qui"; *perse* > *φerse* Gr. Περσεύς; *aφers* (*a.* Etr.) > *afrs* (*n.* Etr.); *fasti* > *hasti.* (Cf. Merlo, 1927, 1950, 1953, 1954, 1957.)

Furthermore, if, as Pallottino adds, in the latest period there appears a phonetico-graphical fluctuation among the various types: e.g. *zic* and *ziχ* "to write", *tui* and *θui* "here", *suti* and *suθi* "repository" (burying place), *uple* and *uφle* etc., this fact can only confirm the presence of aspirated consonants.

The different conditions in which the Etruscan phenomenon appears in reference to the Tuscan facts might prove a considerable difficulty. There are aspirated initial and medial consonants, but the intervocalic position does not appear to be essential, as it does in Tuscan. It must not be forgotten that the primary intensity stress on the first syllable of words, produced in Etruscan the disappearance of many medial vowels so as not to allow, in many cases, the primitive conditions to appear. (Such is the case, to quote only one instance, of the proper name "aχmemrun" in which the aspirated consonant does not appear to be intervocalic, though it must have been so if the word is a loan from the Greek Ἀγαμέμνων). Battisti (1927), moreover, pointed out that in materials of Greek origin traces of aspirated initial consonants are wanting, while aspirated medial consonants are to be found. (Battisti relies more particularly on the material studied by Devoto, 1927.) Despite these facts, however, it is impossible to deny that aspirated consonants do not appear in Etruscan in the same conditions as they do in Tuscan. Aspirated consonants, nevertheless, do exist in Etruscan. Now, it is evident that the presence of aspirated consonants in Etruscan, which is amply documented, is more than sufficient for supposing the tendency to the aspiration of plosives to exist in that language.

A second point that must be observed is that there is not a perfect correspondence between the phenomenon and the Etruscan area. A part of southern Etruria, the one beyond the river Ombrone comprising Mount Amiata, the district of Montepulciano, the Val di Chiana and the country round Arezzo and Siena have no aspiration. But it happens to be just the part of the Etruscan territory that was the first to fall under the sway of Rome. Mount Amiata rises on the linguistic border between Tuscan and the dialects of Umbria and Marche, and even the dialects of Val di Chiana, Arezzo, Castelnuovo and the dialects of the district of Montepulciano have an Umbrian and Senonian substratum. Also the dialect spoken in the Casentino proves to be

more Umbrian than Florentine (Merlo, 1927, pp. 5, 6 of abstr.). In the extreme north-western point of Tuscany, in the territory that extends towards districts with a Ligurian substratum, the phenomenon gets more and more sporadic. I made an investigation at Viareggio to determine the present situation and ascertained that both forms *amio* and *amico*, *domenia* and *domenica* are heard. A similar situation was found by Giacomelli (1934) at Camaiore. The people who were questioned wavered between *k* and *h*.

As to Corsica, it is not surprising that no aspiration is found there. The substratum is different, and even if the imported Tuscan had aspirated consonants, there must have been a reaction owing to which proximate consonants took the place of the aspirated ones.

There remains what is apparently the most serious objection of all: the late evidence for Tuscan aspiration both in testimonies and in spelling. The phenomenon dates back to 1525 to the "Polito" of Tolomei. Some years ago, Folena (1952) seemed to make a decisive contribution observing that Dei, a traveller and memoir-writer of the 15th Century, frequently wrote an *h* in the place of an intervocalic *c* (*poha fede*, *oha*, *amiho*, etc.). With this evidence the first certain, though rather strange, proof of the phenomenon (strange because it even related to the voiced consonant *g*) was antedated. Recently, however, Folena (1956) has been obliged to retract his statement. On better examination of the evidence in Dei he realized that in many other cases *h* is a mere graphical substitute for *ch*. Nevertheless, he pertinently remarks that antedating of two or three generations "could not in itself offer arguments either in favour of the supporters of the Etruscan substratum or in favour of the supporters of free development uninfluenced by substratum". He adds that Castellani (1952) pointed out that the absence of written evidence cannot be taken as a proof that the phenomenon does not exist. For the Tuscan voiced velar intervocalic consonant is a variant of position and cannot be looked on as an independent phone. Folena is

indeed eloquent enough also in other regards. He states (1956, p. 501) that "In Italy, in the humanistic period, the initial Latin *h* was never pronounced, though the problem was formulated; it was not therefore in pronouncing Latin that the habit of pronouncing a voiced velar *h* was formed. In Tuscany this might have favoured graphical reflexes of articulating habits."

It should also be pointed out that, if the evidence of Tolomei and of subsequent grammarians were not there, for many centuries spelling would have taught nothing concerning the aspirated consonant deriving from *k*. No one felt impelled to draw attention to this aspiration, not even Zannoni who wrote his "Scherzi Comici" at the beginning of the 19th Century. In these he points out *-t-* > *-h-* but not *-k-* > *-h-*, which is indeed very significant.

Confronted with this negative but eloquent conclusion, it may seem useless to quote again the Sienese examples of the 13th and 14th Centuries, collected by Hirsch (1885), even if in *diano* for *decano* there seems to be no question of spelling *c*, *h* or *ch*: the intervocalic consonant is actually not written at all.

As for the hypothesis quoted by Hall ("it is a habit that originated at Florence in the 16th Century, and at first affected the guttural plosive *-k-* and possibly also *-g-*"), it cannot be accepted, as Folena (1952, p. 503 *n.*) observed, because Tolomei in 1525 gave evidence that does not refer certainly to Florence alone, but also to Siena, and in 1535 Di Falco spoke of : "the whole modern Tuscan pronunciation which is enunciated with a Hebrew element called *he*"; and, certainly, aspiration will not have blossomed out just at that time!

It is not difficult to answer the arguments advanced by Rohlfs, who maintains that if the phenomenon were Etruscan such enunciations as *pahe*, *nohe*, *vohe*, should be found in Tuscany today. It is easy to object, as I did (Bolelli, 1951), that the fate of a consonant before a velar vowel and before a palatal vowel must be different, but I now add that *c* (o. Etr., also *k* and *q*) before a

vowel *e* appears to have had a tendency to assume a value *ċ* or ç (= *s*), sometimes expressed graphically, with *s* (*ṡ*) (see Pallottino, 1936) as, for instance, *φulnices* and *φulnise*, Gr. πολυνείκης; *face* and *faṡe*.

The conclusion of this presentation [Etruscan survivals in names of places, though interesting (Ribezzo, 1927; Battisti, 1927), have purposely been left out] is clear. In problems of substratum it is ill-advised to expect absolutely certain proofs. If, however, we demand a good margin of probability for solutions provided by linguistics without exacting absolute, mathematically exact evidence, we may consider as very probable the attribution of Tuscan aspirated consonants to the Etruscan substratum, as a tendency to articulate plosives like aspirated consonants which in an intervocalic position acquire the value of actual combining variants without giving rise to positive phonological changes.

Even the objections which have been made against this hypothesis by several linguists have contributed to clarify the facts. Truth, or even approximate truth, always costs much effort, and evidence needs to be most strictly and repeatedly controlled to enable us to look confidently on the development of knowledge.

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DISCUSSION

Banti: The phenomenon of aspiration which Prof. Bolelli has discussed is most interesting, but I do not feel that it can be attributed to the Etruscans. We only find it in the region of Etruria which was very sparsely settled by the Etruscan people, usually the later settlements in northern Etruria. We find it north of the Arno, and it ends at the line where what are considered to be Etruscan place names also end, and where all the Etruscan archaeological finds end. But, as Prof. Bolelli said, it may be due to a special mode of pronunciation of the Etruscan language when this language was spoken by the inhabitants of northern Etruria. At all events, it is interesting that this phenomenon ends at the line where the Etruscan archaeological finds end, and I myself have used it in topographical research into how far north of the Arno there existed Etruscan settlements; but I am not certain that it was an Etruscan phenomenon.

Bolelli: Clemente Merlo's map (1954, *Orbis*, 3, 19) shows two periods, ancient and later Etruria; of course it does not correspond to the whole area of ancient Etruria, but to a large proportion of this zone. So far, it has not been possible to find a perfect linguistic analogy between these two, but who could hope for good agreement after

such a long lapse of time? The Etruscan language can be said to include a phenomenon of boundary. If we do not believe that this aspiration derives from the Etruscan language, at least we cannot deny that it did exist in Etruria. We might think that it originated at a later date. Then there are the phenomena of *nd*>*nn* and *mb*>*mn*. We have been able to obtain reliable information for certain areas, but it would be too much to ask for a perfect correspondence between ancient and modern.

Can the archaeologists and historians tell us what were the successive movements in these particular areas? Can Volterra, for instance, be considered an Etruscan centre or not?

Banti: Yes, but a late one. We have some slight evidence of agricultural centres north of Chiusi, but it is difficult to draw general conclusions from such data.

Bolelli: If Volterra was an Etruscan centre, it does not matter for our purpose whether it was a late one or not, since we are interested in historical, not prehistorical, problems. What about Siena?

Banti: The available evidence indicates that the Etruscans came to Siena at a late date, and remained there for a relatively short period of time.

Bolelli: It seems to me that the phenomenon of aspiration occurred in the area from north of Grosseto along the right bank of the Ombrone. On the left bank there was no aspiration. We have observed some characteristics which recall the dialects of south-central Italy, not the Tuscan dialects, i.e. we have found some linguistic phenomena that are typical for the dialects Oscan and Umbrian. Of course we cannot be sure, but it is reasonable to infer that the Tuscan aspiration derived from the Etruscan language. The fact that this phenomenon is not evenly distributed might be explained in terms of successive infiltrations. It exists in the mountain regions, and I agree with Prof. Banti about these areas; but we must not forget that some of the Etruscans dwelling on the plains may have fled to the mountains to escape subsequent invasions.

Banti: The main objection to your interpretation is that the area you describe in Tuscany is north of the Arno. When the Romans conquered this area it had already been conquered by the Ligurians, so that here we find very little trace of Etruscan civilization, and only for limited periods, not more than 150 years.

Cook: It seems clear that as long as Etruscan was spoken in the area it matters little whether the area was settled late or early, from the point of view of linguistic survival.

Ward Perkins: Would it be true to say that the same factors that have influenced survival since Etruscan times could hypothetically equally well have played a part from pre-Etruscan times? In archaeological terms, one is between two possibilities: (1) that one is dealing with the possible subsequent changes (as Prof. Bolelli suggested), and (2) that these particular phenomena of northern Tuscany could well have been something that the Etruscan language underwent in that particular part of Etruria, as distinct from the south.

Lehmann: Are there any other areas in Italy or in the Mediterranean which show this peculiar feature of aspiration?

Bolelli: No. Theoretically, it is possible that this may be purely a pre-Etruscan phenomenon. For instance, in Etruscan texts we find a *k* which becomes an *h*, etc. The ancient Etruscan language already shows certain common phenomena, whereas aspiration is a phenomenon peculiar to modern Etruria, as philologists know. This seems to be positive proof in support of my theory.

Bullough: In looking through 9th- and 10th-Century documents, I noticed that there is more evidence for this tendency to aspiration (some of it anomalous) than has been used by the philologists who documented it. Some of it could be ascribable to error. Another point is that many Germanic words were introduced, but one finds no plosives.

Bolelli: I don't see why there should have been a change in epigraphy if it did not correspond to a linguistic change. I have based my premises on the material at our disposal. Archaeologists are quite certain that these changes in writing do correspond to a change in speaking; actually, they do correspond to the beginning of aspiration. If we doubt even this, then we have no reliable basis on which to conclude anything.

Bloch: We should not be too sceptical on this subject. It appears that the tendency to aspiration existed in ancient Tuscany, and still exists in modern Tuscany. This is a unique phonetic phenomenon, peculiar to this one region in Italy. It is unlikely that no link could be found between this phonetic peculiarity and the influence of the ancient

Etruscan language. Although we have very little data which could establish clearly that this link exists, Prof. Bolelli's demonstration does seem conclusive; and there remains the evidence of place names which also supports his view.

Banti: My interest in this problem has been from the point of view of comparison of place names, and what I have seen confirms topographically what Prof. Bolelli has said about linguistic patterns. I am sure that there is a link between the phenomenon of aspiration and the Etruscans, but I am not sure that it is a strong one.

Kirk: What is the exact relevance of this point for the problem we are discussing? Would Prof. Bolelli maintain that we can use aspiration as a criterion for determining modern communities which do preserve, better than others, ancient Etruscan elements? Or would he maintain that the survival of this linguistic feature is an indication that there has been little genetical interference with the ancient population of Etruria up to the present day? Unless one of these two things is the case, it seems to me that this question has relatively little bearing on the subject under discussion.

Bolelli: I stated clearly that I was speaking outside the question of the origins of the Etruscans, which is not my sphere.

Morganti: Prof. Bolelli, as biologists we are not concerned with the origins of the Etruscans, but with the people who are now supposed to be descended from them. What does the phenomenon of aspiration tell us about the people of north-eastern Etruria?

Bolelli: Further to what Prof. Banti said, in the area inhabited by the Ligurians we find no trace of aspiration; which again supports my view that aspiration is to be found only as a direct consequence of Etruscan influence. Aspiration is not found in the modern areas corresponding to ancient Ligurian influence.

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THE EVALUATION OF METRICAL DATA IN THE COMPARISON OF ANCIENT AND MODERN BONES

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THE study of the skeleton has occupied a central place in physical anthropology ever since it emerged as a selfconscious branch of biology over one hundred years ago. It is easy to understand why a science so much concerned with the origin and evolution of the human species should have placed such emphasis on the bones and skulls which are the most direct, tangible evidence of the physique of vanished populations. It followed as a natural consequence that large collections of modern skeletons were accumulated for comparison until there may have been some truth in the popular view that physical anthropology was concerned with nothing else but the tedious examination of dry bones.

In this paper we shall first discuss some generalities about osteometric work and we shall then present some material bearing on the problem of Etruscan origins, which is the special interest of this symposium. We cannot claim any expert knowledge of this latter subject, and in the time at our disposal we have had to confine ourselves mainly to assembling and reconsidering old data. The generosity of the Ciba Foundation has, however, enabled Mr. Brothwell to measure some thirty undescribed Etruscan skulls in the Musée de l'Homme, Paris, and we are grateful to Professor Vallois for giving us access to this collection. Dr. Oakley has also kindly allowed the British Museum material to be included.

In recent years there has been a distinct swing of interest away from the osteometric tradition. This has been largely due to the discovery of widely distributed and simply inherited traits, among which the blood groups are the most fully explored. The whole subject of population genetics, backed by a powerful theoretical apparatus provided by statisticians, has become an important branch of biology and has naturally influenced many students of human populations. The geneticist who is striving to achieve clarity in so complex a field is not likely to be attracted by skeletal measurements, though Dr. Siniscalco (this symposium, p. 205) will show us that there can be exceptions to this rule. The kind of skeletal variation with which anthropologists deal is mainly a matter of small, graded differences in proportions; the inheritance of such quantitative characters seems to depend on the combined influence of many genes, the individual actions of which cannot usually be discerned. When we are studying a character such as a blood group, we can be reasonably sure that we are dealing with the same gene wherever we find it, but we cannot be at all sure that apparently similar skeletal forms have the same genetical basis in different populations. In addition, skeletal measurements are likely to be affected, to an undetermined extent, by environmental factors acting within the lifetime of the individual.

If the various attempts to determine blood groups on ancient bones should ultimately lead to the perfecting of reliable techniques, we should then have direct evidence of the incidence of simple genetical traits in extinct peoples. At the moment, our attempts to reconstruct past events from the present distribution of blood groups necessarily involve various assumptions which may well be oversimplified and may be difficult to check by independent evidence. The bones, with all their limitations, have the merit that they are actual fragments from the past which have endured. The situation is much the same as in animal biology, where evolutionary schemes based on a comparison of

living forms, which are terminal products of a process of change, are more speculative and insecure than those founded on adequate fossil material.

It would be a pity if, in the enthusiasm for the genetical era in anthropology, the testimony of the bones were to be neglected. There is some danger of this; anatomists who at one time undertook so much of the descriptive work on archaeological material tend nowadays to be attracted to other fields nearer the borders of physiology. Morphologists can be found to describe the rarer and more spectacular fossil finds relating to remoter phases of human evolution, but the examination of the much more numerous skeletons from comparatively recent periods is more laborious and perhaps less immediately rewarding. Archaeologists seldom have the time, training or perhaps the inclination to busy themselves with bones once they have noted the method of burial, and regrettably in their writings we often find that they deal exclusively in terms of the change and spread of cultures with little or no reference to the biological events which may or may not accompany them.

The traditional aim of osteometry is to describe the skeletal form of various populations, both ancient and modern, and then by considering their resemblances and differences, to suggest the most plausible scheme of interrelationship between them. Since it is unsatisfactory to rely on subjective impressions, the form is best described by a set of measurements. The human skull is a highly complex shape in detail and it is obvious that all its subtleties cannot be expressed in this way without taking an inconveniently large number of measurements. The school of Karl Pearson, to which osteometry owes so much for its prolonged and careful work guided by statistical concepts, routinely took thirty or more measurements, but since many of them are rather highly intercorrelated it is doubtful whether the extra information gained justifies the additional labour. Different conventions of technique and inadequate descriptions of method

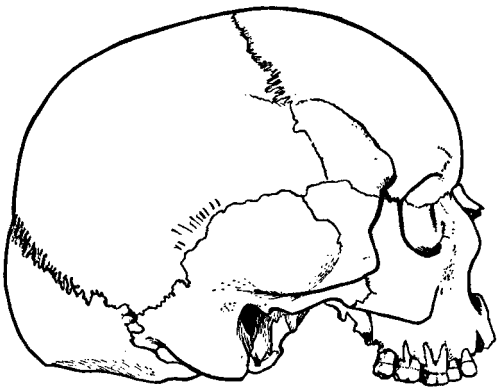

FIG. 1a.

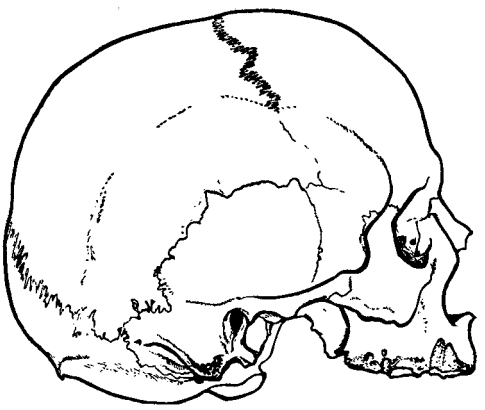

FIG. 1b.

are a frequent source of trouble which often obliges one to reject otherwise useful data.

The rise of statistical methods has made us particularly cautious in drawing conclusions from insufficient material which may give an inadequate picture of population variability. One sometimes wonders whether earlier archaeologists with an over-riding interest in the richer tombs may not have dug in a way that would give an unrepresentative view of the population as a whole. We

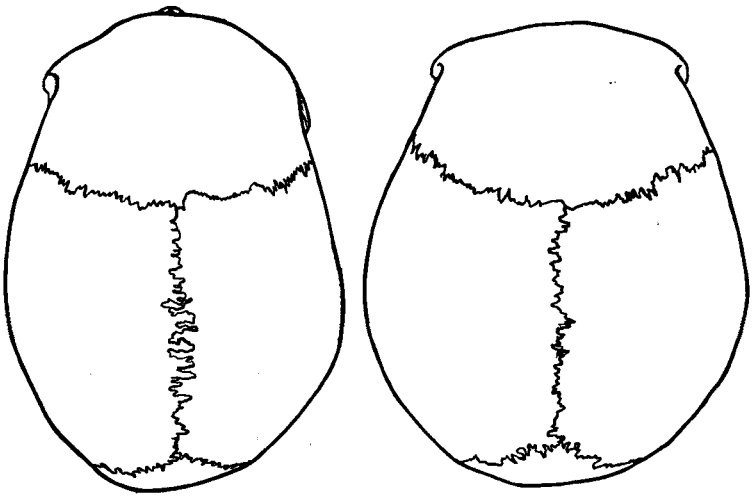

FIG. 1c. FIG. 1d.

FIG. 1. Profiles of 2 skulls from the British Museum, to illustrate how much variation can occur even in a small collection.

should certainly prefer to have all the skeletal material from a site rather than a few specimens which may not be characteristic or, at the worst, may be selected under the influence of some preconceived theory. Even a large collection of skulls, however, is bound to contain many irreparably damaged specimens and the number which will yield a full range of measurements may prove to be lamentably few.

All populations are very variable in skeletal measurements, as in other features, and we must abandon the earlier view that "pure" races in which this variability would be substantially reduced can be found (Morant, 1935). In Fig. 1 are shown profiles of two skulls, catalogued as Etruscan, from the British Museum to illustrate how much variation can occur even in a small collection. In Fig. 2 we have plotted out the head breadths of a sample of male Etruscan skulls pooled from various publications and we

have set beside it the distribution of the same measurement in the Sardinians (Duckworth, 1911) who are the most divergent, in this respect, of the various Mediterranean series we have collected for comparison. The range of variation within each of these populations is considerably greater than the difference between the mean values. Usually, if we are considering a restricted region,

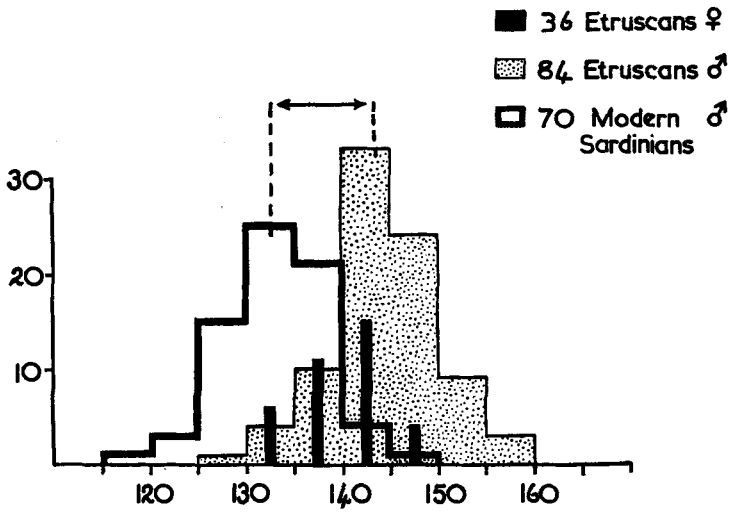

FIG. 2. Distribution of maximum head breadth (mm.).

the difference between population means is a matter of millimetres as may be seen in Table I, while the variation in major skull dimensions within each of them is one of centimetres. We are, therefore, always confronted with differences of a statistical nature and, if these are small, quite large samples may be needed before we can be confident that they are not due to chance in sampling but are indicative of real biological divergence.

In Fig. 2 the distribution of head breadth in a small pooled sample of Etruscan female skulls is also included and, although there is great overlap with the male series, the mean is distinctly lower. Since there are, in fact, systematic differences in size and

Table I

MEAN SKULL MEASUREMENTS (mm.) OF POPULATIONS COMPARED BY DISTANCE STATISTICS

(The number of measurements is given in brackets)

	L	B	H′	GL	G′H	J	N′H	NB	O_2	O_1
1. Etruscans, M	184·3	143·3	134·8	95·0	71·2	131·6	51·5	24·3	32·8	39·2
	(188)	(190)	(151)	(87)	(148)	(117)	(150)	(137)	(112)	(112)
2. Etruscans, F	178·7	138·8	128·9	91·0	67·8	131·6	49·4	23·7	32·9	38·0
	(76)	(73)	(67)	(40)	(53)	(117)	(63)	(55)	(48)	(49)
3. Basques, M	185·8	143·5	130·8	90·7	70·7	128·8	51·9	23·2	33·6	41·2
	(39)	(39)	(39)	(37)	(31)	(34)	(37)	(35)	(35)	(35)
4. Pompeian, M	181·6	142·6	131·3	93·5	70·7	—	52·2	24·1	34·1	—
	(55)	(55)	(50)	(50)	(54)		(55)	(55)	(55)	
5. Roman, M	181·4	143·0	133·5	—	70·4	132·6	—	—	33·6	—
	(21)	(21)	(21)		(21)	(21)			(21)	
6. Emilian, M	177·4	143·2	130·1	—	69·0	131·3	51·8	24·4	—	—
	(27)	(27)	(27)		(27)	(27)	(27)	(27)		
7. Sicilian, M	180·7	136·8	132·1	—	—	—	49·2	23·4	31·6	—
	(107)	(107)	(106)				(103)	(103)	(107)	
8. Sardinian, M	185·2	132·3	130·6	96·5	68·5	127·7	50·0	24·2	—	—
	(70)	(70)	(63)	(43)	(45)	(59)	(57)	(61)		
9. Sienese, M	176·0	141·1	131·4	—	—	—	—	—	—	—
	(50)	(50)	(50)							
10. Greeks, M	176·4	143·1	136·1	94·8	71·8	132·1	51·8	24·5	34·3	—
	(95)	(95)	(92)	(91)	(93)	(91)	(77)	(76)	(93)	
11. Greeks, M	185·4	140·1	132·9	93·2	68·4	129·2	49·5	24·0	33·1	38·9
(Mycenaean)	(37)	(39)	(26)	(16)	(17)	(20)	(19)	(19)	(19)	(18)
12. Greeks, M	184·0	141·8	133·7	94·8	68·0	132·5	50·2	24·3	32·7	39·5
(Iron Age)	(25)	(25)	(22)	(16)	(19)	(19)	(20)	(20)	(20)	(19)
13. Cephallenian, M	188·5	138·9	133·4	99·2	65·3	129·6	47·6	24·5	31·2	37·9
	(25)	(27)	(18)	(9)	(11)	(13)	(13)	(14)	(14)	(14)
14. Turks, M	175·4	145·8	135·9	94·7	71·9	134·0	—	—	34·7	—
	(78)	(78)	(70)	(66)	(66)	(64)			(65)	
15. Egyptians, M	185·3	138·9	133·1	95·0	70·4	128·7	50·85	24·4	33·7	—
(26–30th dynasty)	(895)	(896)	(884)	(832)	(845)	(785)	(898)	(893)	(888)	
16. Cretan, M	180·1	139·6	137·3	—	70·1	129·7	50·7	24·2	—	—
	(52)	(52)	(51)		(40)	(51)	(49)	(49)		
17. Maltese, M	180·5	139·0	136·6	—	68·1	132·3	51·3	24·4	33·3	41·5
	(467)	(468)	(461)		(437)	(263)	(461)	(452)	(456)	(458)
18. Cyprus, M	175·0	144·4	135·9	98·4	67·7	—	49·6	23·4	32·6	39·4
(Bronze Age)	(21)	(20)	(18)	(16)	(16)		(17)	(17)	(17)	(16)
19. British, M	187·4	141·4	132·9	96·3	69·1	130·6	50·6	23·7	33·6	—
(Iron Age)	(61)	(102)	(77)	(63)	(30)	(55)	(99)	(67)	(65)	
20. Ashanti, M	183·2	131·8	135·6	101·7	67·5	128·4	49·3	27·9	33·9	42·6
	(45)	(45)	(45)	(23)	(24)	(35)	(43)	(36)	(43)	(35)

L = maximum length of braincase
B = maximum breadth of braincase
GL = basion-prosthion length
G′H = upper facial length
H′ = basiobregmatic height (see Morant, 1928)
J = bizygomatic breadth
N′H = nasion to base nasal spine
NB = nasal breadth
O_2 = maximum orbital height
O_1 = orbital breadth (see Schmidt, 1887)

References: Samples 3, 7, 8, 9, 10, 14, 15, 17 and 19: Morant (1928); 4: Nicolucci (1882); 5: Sergi and Capritti (1928–29); 6: Graffi (1928–29); 11, 12 and 13: Angel (1943); 16: von Luschan (1913); 18: Fürst (1933); 20: Trevor (1949).

form between male and female skulls, one of the preliminaries to osteometric comparison is a sorting into sexes. The method of doing this is not always stated in publications, but it usually depends, in fact, on a subjective assessment of general size and of the degree of development of certain bony prominences. There are likely to be a number of ambiguous specimens, however, and it is doubtful whether, in many collections, an accuracy of more than about 90 per cent can be expected (Keen, 1950). Certain other bones, notably the pelvis and femur, are probably more useful for sex identification.

If sufficient numbers of skeletons of known sex are available we may take advantage, as Pons (1955) and Thieme and Schull (1957) have done, of an advance in statistical technique due to Fisher (1936) to get a more efficient sorting. This method, known as discriminant analysis, makes use of the fact that although classification may be inaccurate when only a single measurement is employed, we may do better if we can combine the information derived from several. Some uncertainty may arise when we are dealing with extinct populations but serious errors are unlikely if we employ the statistical constants worked out for those modern peoples which most closely resemble them.

The mention of sexing procedures should remind us that this problem, together with the determination of age and stature from skeletal remains, are matters of perennial interest in forensic medicine. Much of the basic material has been collected by anthropologists and anatomists, and valuable work has been done in the last ten years (Trotter and Gleser, 1952; Telkkä, 1950). Karl Pearson was a pioneer in applying the statistical methods of regression analysis to the prediction of stature from the length of the long bones.

A question that often arises when we are examining a collection of skeletons is whether the population is homogeneous or whether it contains a number of distinct types. It is reasonable to expect that many ancient communities, which may have included quite

large proportions of slaves traded from distant regions or captured in war, would show such heterogeneity. Earlier anthropologists were much devoted to a typological approach and placed great weight on the finding of apparently aberrant skulls which they took to be evidence of some intrusive or relict stock. The well-known Italian authority, Giuseppe Sergi (1933–34), believed that he could detect the presence of three groups, which he presumed to be of different origin, in his collection of skulls from Etruria. From the metrical point of view we should expect a heterogeneity to result in the distortion of the distribution of a relevant measurement from the symmetrical form and, if it were sufficiently pronounced, to lead to actual bimodality. As we have already shown, however, in the case of the distribution of head breadth, we are often dealing with differences too small to give a decisive effect and, in any case, statistical testing of distribution form is not rewarding unless rather large samples are available.

The diagnosis of a divergent type is usually made, however, not on the basis of one measurement, but of a combination of many; that is to say, on the "total morphological pattern" as Le Gros Clark (1955) has aptly termed it. As far as we are aware, there is no really objective statistical device for detecting such heterogeneities with respect to multiple measurements. By way of simple illustration we have plotted out the maximum skull lengths and breadths for our pooled sample of Etruscan males and females (Fig. 3). The points are fairly uniformly scattered and there is no evidence of the clustering which one would expect if the sample comprised two or more populations differing markedly in their general head shape. In expressing this view, however, we are merely giving a subjective opinion about the scatter of the points on the graph instead of about the skulls themselves, and we have done no more than to display some of the facts in a convenient and simplified way. If, of course, we had *a priori* reasons for believing that the material was heterogeneous and we had measurements on each of the component groups, then

the problem would be similar to that of sorting sexes, and we could use the discriminant technique to arrive at the best classification of individual specimens.

The provenance of the majority of the Etruscan skulls described

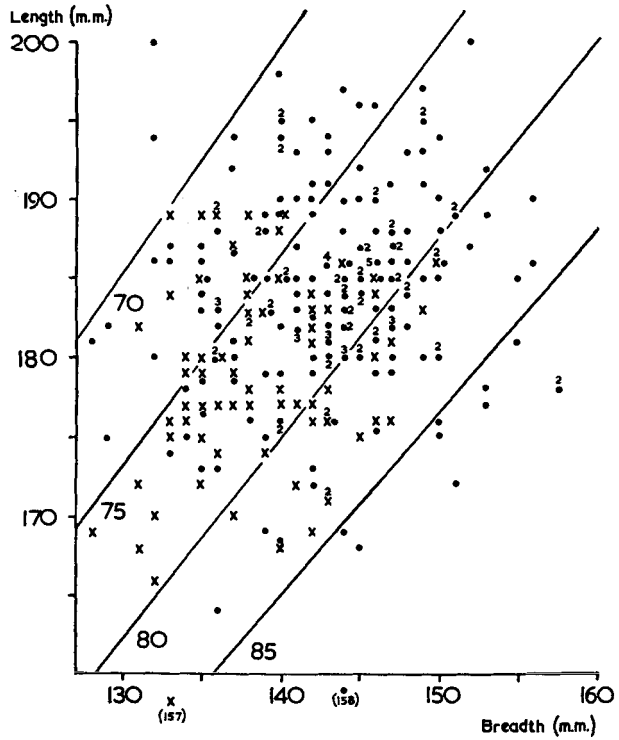

FIG. 3. Distribution of maximum skull length and breadth.
Diagonals are lines of equal cephalic index.
Etruscans ♂ (190) = ● Etruscans ♀ (72) = ×

in the literature is given, and it seemed worth while grouping them regionally in order to look for evidence of differences. We have, therefore, pooled those from the south, which come mainly from Tarquinia, for comparison with those from the north, many of which come from Chiusi. Table II shows the

mean values of ten measurements on the skull for these regional groups and also for the total sample of females. The measurements cover the major dimensions of the braincase and face including the orbits and nasal skeleton, and for some of them the

Table II

MEAN SKULL MEASUREMENTS (mm.) OF POOLED ETRUSCAN SAMPLES
(Number of observations in brackets)

(The number of measurements is given in brackets)*

	L†	B	H′	GL	J	G'H	N'H	NB	O_2	O_1
Males										
South	183·9 (130)	143·5 (130)	135·3 (107)	95·0 (75)	131·6 (90)	71·6 (114)	51·6 (105)	24·2 (99)	32·8 (93)	38·9 (93)
North	185·1 (57)	142·8 (60)	133·5 (44)	96·2 (15)	131·5 (27)	70·0 (34)	51·3 (45)	24·3 (38)	32·8 (19)	40·9 (21)
Total Males	184·5 (187)	143·3 (190)	134·8 (151)	95·6 (90)	131·6 (117)	70·8 (148)	51·5 (150)	24·3 (137)	32·8 (112)	39·9 (114)
Females	178·7 (76)	138·8 (73)	128·9 (67)	91·0 (40)	124·7 (43)	67·8 (53)	49·4 (63)	23·7 (56)	32·9 (48)	38·0 (49)
	Standard deviations ($\hat{\sigma}$) of skull measurements									
Males										
South	7·53	5·27	6·21	5·66	5·49	4·01	3·18	1·66	2·16	1·78
North	6·34	6·14	5·84	6·81	4·60	3·29	3·15	1·83	2·35	2·63
Total Males	6·41	5·54	6·14	5·74	5·29	4·95	3·18	1·70	2·18	1·92
Females	4·56	4·95	5·36	6·16	4·26	4·12	2·88	1·93	1·19	1·92

* The samples are pooled from data in Schmidt (1887), Sergi (1915), Zannetti (1871), Mosso (1906), Angelotti (1909–11), Frassetto (1905–06), together with data from the British Museum and Musée de l'Homme.

† Abbreviations as in Table I.

numbers are small. The variability of the measurements expressed as the standard deviation is shown in the lower part of Table II. The regional means are closely similar, and if there are differences, the samples are too small to substantiate them; nor is there much evidence of differences in variability.

The next question to be asked is which populations do the Etruscans most resemble when the totality of their skull measurements is taken into account? The coefficient of racial likeness

devised by Pearson (1926) and extensively used by his school was designed to deal with this problem. A more efficient distance statistic, as it is called, was later derived by Mahalanobis (1936) and Rao (1948) and used in their comparisons of Indian castes and tribes. Since we generally have no definite grounds for believing one measurement to be essentially superior to another as an index of the affinity of populations, all the measurements available are given equal weight and are combined to give a single figure which is a measure of the degree of divergence between each pair of populations. A useful modification of this procedure was introduced by Penrose (1954); it is not only very simple to compute but has the advantage of splitting the distance measurement into two components which he terms "size" and "shape". The former is a measure of divergence in overall dimensions and the latter of divergence in the relative magnitudes of the measurements.

Morant (1928), in his comparison of European skulls from many sources by the coefficient of racial likeness, includes some Etruscans in his material. We have enlarged the Etruscan sample and have recalculated the distance of various other populations from it using the Penrose method. We have confined ourselves mainly to various populations, both ancient and modern, from the Mediterranean area (Table I), and have included one or two more recent samples in addition to those given by Morant. The choice of material is, nevertheless, somewhat haphazard and arbitrary; we could wish for more from other regions and periods in Italy itself, and the Middle Eastern area which tradition links with Etruscan origins is quite inadequately represented. The differences in the mean values on which the comparisons are based are often small and we have included a sample of West African skulls from Ashanti in order to give an idea of the scale of differences on which we are working. We have not considered the larger question of the relationship of all these populations to each other but only the relationship of each of them to the

Etruscans (Table III). In Figs. 4 and 5 the Etruscan males are therefore represented at the origin of the size and shape scales. The first figure is based on a comparison of the three major dimensions of the braincase only, since these are measurements

Table III

DISTANCES OF VARIOUS POPULATIONS, FROM ETRUSCAN MALES, BASED ON SKULL MEASUREMENTS

	*L, B, H′ *only*		*All available measurements*		*Number of measurements*
	Size distance	*Shape distance*	*Size distance*	*Shape distance*	
(Po) Pompeian	0·171	0·045	0·013	0·136	8
(Ro) Roman	0·068	0·030	0·010	0·235	7
(Em) Emilian	0·462	0·229	0·124	0·216	7
(Sn) Sienese	0·678	0·161	—	—	
(Si) Sicilian	0·656	0·137	0·174	0·269	6
(Sa) Sardinian	0·922	0·982	0·322	0·566	8
(Ma) Maltese	0·205	0·192	0·048	0·183	8
(CyBr) Cypriot (Bronze Age)	0·212	0·600	—	—	
(Cr) Cretan	0·107	0·318	0·038	0·174	8
(Gr) Greek	0·134	0·459	0·010	0·319	9
(GrM) Greek (Mycenaean)	0·077	0·118	0·165	0·092	7
(GrI) Greek (Iron Age)	0·036	0·012	0·051	0·094	7
(Ce) Cephallenian	0·022	0·432	0·249	0·512	7
(Tu) Turkish	0·064	0·767	0·010	0·492	7
(Eg) Egyptian (26–30th dynasty)	0·120	0·191	0·030	0·138	9
(Ba) Basque	0·028	0·008	0·015	0·174	9
(BI) British (Iron Age)	0·005	0·055	0·010	0·129	9
(Et) Etruscan females	0·935	0·001	0·436	0·122	9
(As) Ashanti	0·629	1·230	0·006	1·546	8

Note: The mean values were standardized by dividing by an estimate of the standard deviation of each measurement. This estimate was obtained by pooling the variances of three long series without weighting; the series were: 1. Egyptians (26–30th dynasty); 2. Naquada; 3. British as given in Pearson (1926).

* Abbreviations as in Table I.

which are nearly always available. Many of the populations differ very little from the Etruscans in size but are more or less divergent in shape; the Etruscan females, however, are almost entirely distinguished by the size measurement, being notably smaller as inspection of the mean values shows. The recent Italian samples

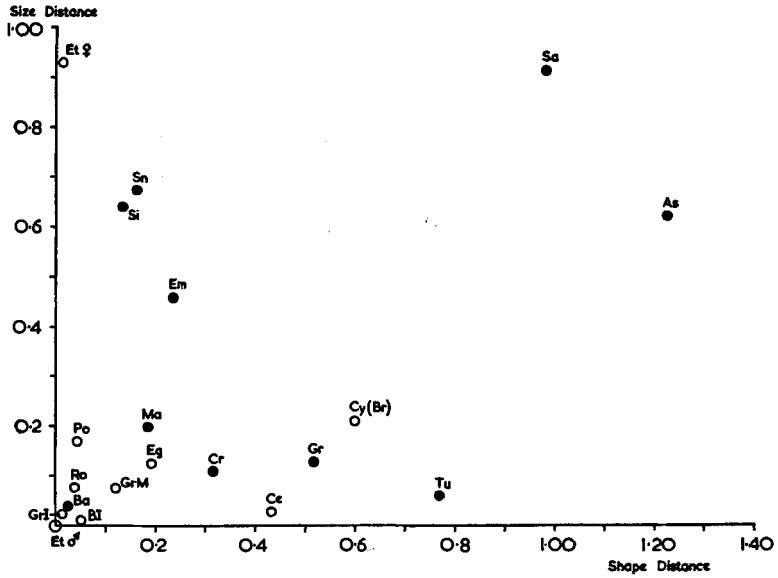

FIG. 4. Distances of certain Mediterranean peoples from Etruscans. Based on skull measurements L, B and H′ (see Table I) only. (Males)
○ = Ancient. ● = Modern.

from Emilia, Siena and Sicily are also differentiated, mainly in size, from the Etruscans; and it is interesting to find that the Pompeians and a small collection of recent Romans are close to them in both dimensions, but so are the Basques and both Greeks and British from the Iron Age, the latter being a sample from the south of England in the Roman-British period. The recent Sardinians with their small and relatively narrow skull are almost as far away as the Ashanti. Unfortunately, the whole

battery of measurements is not available for many samples, and different ones are missing in different cases. With some misgiving, we have prepared a second diagram (Fig. 5 and Table III) showing the distances when all the available face measurements are added. When this is done, some of the nearest groups (Romans, Pompeians, British and Greek Iron Age) move a little further away in

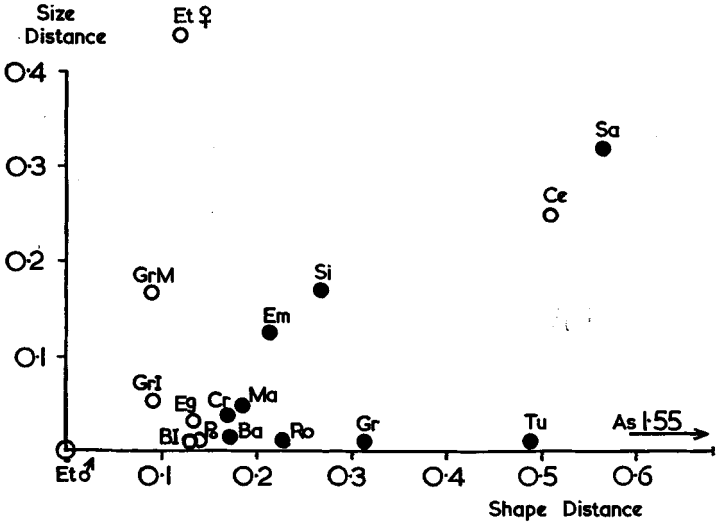

FIG. 5. Distances of various Mediterranean peoples from Etruscans. Based on all available skull measurements. (Males)
○ = Ancient. ● = Modern.

shape, as do the Basques who, as Morant (1929) noted, have some rather extreme face measurements. The general effect of adding more characters is to reduce the size differences, though the Sardinians are still very divergent, while the Ashanti with their broad nasal aperture and projecting maxilla become even more distant in shape but barely distinguishable in size.

An examination of skeletons need not, of course, be restricted to the kind of comparisons we have just illustrated. Observations can also be made on the incidence of certain diseases which leave

their mark on bones or teeth, and these may provide a sidelight on conditions of life in ancient communities. Mr. Brothwell noted that about 4 per cent of the teeth in the Paris Etruscan collection were carious, and a similar figure has been reported for ancient Roman and Egyptian skulls (see Table IV). Resorption

Table IV

DENTAL CARIES INCIDENCE IN ETRUSCANS (Paris) AND OTHER GROUPS
(Estimated on number of permanent teeth in adult skulls)

Population	*Number of carious teeth*	*Number of teeth examined*	*Percentage of carious teeth*	*Number of skulls*
1. Etruscans	20	427	4·6	32
2. Romans (ancient)	8	103	7·7	15
3. Recent Italians	19	246	7·7	25
4. French (Neolithic)	19	258	7·3	34
5. Egyptians (ancient)	82	1805	4·5	137
6. Recent Greeks	24	378	6·3	108
7. Recent Yugoslavs	37	109	34·0	68

1, 2, 3, 4: Original observations. Musée de l'Homme Collections.
5, 6, 7: From Klatsky and Fisher (1953).

of the tooth sockets indicative of paradontal disease was noted in half of the thirty-two skulls and about one-third of them showed hypoplasia of the dental enamel.

The conclusions from our survey must necessarily be tentative; it is possible that the addition of more measurements derived from larger samples might modify the results to some extent. As far as we can discover, only very few Etruscan skeletons have been described in entirety (Messeri, 1953–54*a*) but the size differences in the skulls probably reflect overall differences in body size to a large extent. We are impressed, on the whole, by the degree of similarity between the Etruscan skulls and those of several other populations, including some from modern Italy, and we can see little reason to suppose that the Etruscans were a highly differentiated, and perhaps intrusive, local type. In this view we agree with Messeri (1953–54*b*), one of the most recent writers on

the problem from the physical standpoint. It may be, as some writers have suggested (Pallottino, 1955), that the emergence of a distinctive Etruscan civilization was a matter of local cultural evolution which did not involve much population change.

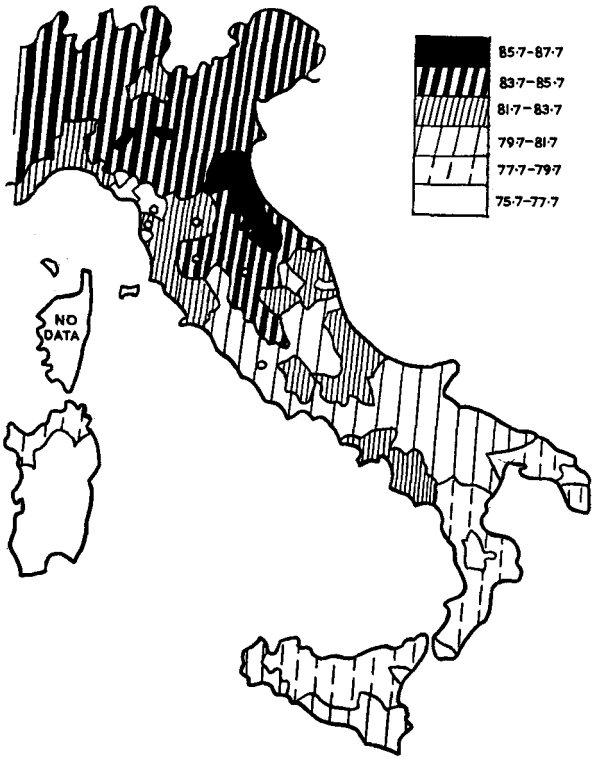

FIG. 6. Cephalic index in Italy (modified from Livi, 1905).

On the other hand, any movements of population which did occur may have involved peoples who were already too similar in physique for the change to be detectable.

We should like to draw attention, finally, to the results of Livi's (1905) anthropometric survey of Italian recruits early in this century. Fig. 6 shows a simplified version of his map of the

cephalic index; the former Etruscan territory is not noticeably distinguished but participates in the general gradient of increasing brachycephaly as we pass from south to north. A wedge of high indices encroaches on the eastern part of the Etruscan zone and there is a patch of low indices in the vicinity of Lucca. When we also take into account Livi's maps of stature and pigmentation, we are left with the impression that the modern population of Etruria shows little evidence of divergence from its neighbours.

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DISCUSSION

[At this point Dr. Oakley presented a small exhibition of skulls arranged by the British Museum (Natural History).—Eds.]

Oakley: In our collection at the British Museum we have some twelve skulls from ancient Etruria. The credentials of these skulls vary considerably, and only five or six of them can with certainty be said to be skulls of Etruscans. For instance, in one case the register states: "Skull of an ancient Etruscan, dug up in a field 6 miles southwest of Chiusi. With it were found some fragments of black pottery". There is a further note stating that the evidence is not sufficient to prove Etruscan origin, but that nothing is inconsistent with that conclusion. On the other hand, three of the specimens that I have here are definitely stated to be from Etruscan tombs.

We have here one allegedly Etruscan skull, whose preservation and morphology are so different from those of the others that one suspects that it may not be so ancient. It is written on and said to have been found at Vulpi in 1878: but one cannot help wondering if dealers had some part to play in the provision of material for Museum collections in those days.

The condition of preservation of these skulls is an aspect I would like to enlarge on, because in the Anthropology Section of the British

Museum (Natural History) we are particularly interested in what one might call the non-metrical features which can be used to provide information about ancient populations, e.g. the possible determination of blood groups in ancient material. My colleague, Mrs. Madeleine Smith, will discuss the techniques which she is investigating for the testing of ancient bones for blood group substances. She has, in fact, carried out preliminary tests on two of these Etruscan skulls. Dr. Barnicot has already mentioned the degree of selection that occurs in building up bone collections. One of the most unfortunate selections is that crania have often been collected with the exclusion of other skeletal material. This is so because anthropologists of the last century were rather inclined to look at single skulls as though these would give a picture of the ancestry of the individuals which they represented. We now know that you can gather very little from individual skulls; and that you need a large series to be analysed metrically, as Dr. Barnicot has shown. The selection of skulls has been quite extreme in the case of Etruscans. We have not one postcranial bone of Etruscan origin in our rather vast human osteological collections. Even in the Musée de l'Homme, Paris, where they have three or four times as many Etruscan skulls as we have, there are very few limb bone fragments. This deficiency is particularly unfortunate in connexion with the use of blood grouping techniques in the study of ancient populations. As Mrs. Smith will indicate, the best material for testing is in the marrow tissue of the limb bones. I hope that our Italian colleagues, biologists and archaeologists, will be able to provide material which will help us to carry out this research. It is very important that the material should not be treated with preservatives, which have indeed rendered much of the material in the older collections unsuitable for blood-grouping.

Smith: To begin with, I shall give a very brief summary of the present position. So far it has only been proved possible to type within the *ABO* system. It is very difficult to say whether we shall be able to improve on that. We hope we shall. The original research in this field was done in the 1930's by Boyd and Candela and their co-workers in the United States. Their technique, with a number of modifications which they themselves suggested, is the one which is in use now. Both these groups of workers show an essential similarity in their basic technique; and I shall outline that very simply.

This is an inhibition technique which means that serum is applied directly to a measured bone sample. The theory of this test is that group substance present in the bone will inhibit the action of its appropriate antiserum. This can then be removed and tested against fresh red blood cells of the appropriate group to see if its activity has been in any way affected. The modifications of that technique include the use of four naturally occurring antisera, and one immune or artificially produced antiserum, in each group, on each of the samples. The disadvantage of this is that considerable quantities of material are necessary, and in the case of the Etruscan skulls which we tested, we were only able to obtain something like 0·8 g. from the mandibles of two skulls, which were suitable for testing. So these samples have only been put up with one antiserum each of type *A* and type *B*.

Cancellous bone is the type which we require for this test. Compact bone has been shown to have little if any group substance present. Therefore, we are not able to work with precision on material from skulls. Ideally, what we would require are long bones, especially the femur. The Etruscans were drilled on the mandibles and, of course, the quality and quantity of the material there is poor.

The disadvantages of the techniques that I use at present are that the serum, when applied directly on to the bone, is not only subject to the action of blood group substance, but is also subject to the action of any other substance present on the bone at the same time, e.g. dirt, which almost invariably accompanies this kind of material. Preservatives present us with a very big problem. We had four mandibles from which we could have taken samples; two of them I did not attempt, because one at least was full of paraffin wax and the other had been lacquered. More serious perhaps is the possibility of the presence of bacterial antigens. It is very difficult to distinguish between the action of bacterial antigens and the presence of certain blood group substances. The problem on these two particular samples was that we obtained non-specific reactions. These are identical with reactions for group *AB* which, of course, occur only very rarely. Therefore, the fact that this reaction occurred on these two samples is open to very considerable suspicion. Occasionally it is possible to absorb out that non-specific reaction with the first lot of serum. Solutions to the problem of non-specific reactions have been suggested along those lines, but they are not

yet confirmed. The danger of trying to absorb out with any serum is that sera invariably contain small quantities of blood group substances, so that subsequent application of different sera may be affected by the first application.

There is one other problem which is particularly applicable to the Etruscan material. As yet we have no sure method of positive identification of group *O*. At the moment it is defined as the absence of *A* and *B*. It may be due to degeneration in the sample. That is a problem for future research, with perhaps plant agglutinins. Its importance here is that if we found a low incidence of group *O*, it would distinguish the Etruscans from known old European populations, such as the Basques, who have a high *O* frequency. If, on the contrary, we found a high *B* frequency amongst the Etruscans, it would then support the theory that the Etruscans originated further east.

A great deal of what we hope to achieve in this field is very much in the future. We now need to perfect the old methods and also to find new ones. We feel that we are very fortunate in London, because we have the advice of many experts in this field. Dr. Mourant, Dr. Lehmann and Prof. Morgan of the Lister Institute are all helping us very much indeed. We hope that in a short time, with that assistance, we may have something more positive to say on the subject.

The kind of material which we require is outlined in appendix I, p. 246. Preferably, it should be placed as soon as possible after excavation in a well-stoppered container, so that dust and other impurities cannot reach it. The quantity which we require should be enough to yield at least 1 g., ideally 5–10 g., of cancellous bone.

As regards the type of future programme that we are planning, we have obtained a long series of femora from a site in Farringdon Street in the City of London, and we have been discussing the possibility of blood typing these femora and combining the results with a report on such features as stature, the incidence of disease, and other similar ideas. This is quite a recent series, and for that reason we are interested in it because we propose to work back in time until we reach the limits of the technique; and we hope this will take us as far back as the Etruscans.

Cook: How can you preserve bones without preservative? There must have been many occasions when bones turned up in a damaged condition.

Smith: Plaster would be much better than paraffin wax or lacquer.

Brothwell: Could not a sample be taken before the bones are treated? In some cases it is quite evident that considerable fragmentation will take place if a preservative is not used. I think the application of an Alvar solution is preferable to using plaster, wax or lacquer.

Smith: I think that is the ideal arrangement.

Cook: Does it matter if they do break up?

Oakley: No, the important thing is to keep the material of each individual isolated. If you find a single skeleton, even if it is very fragmentary, or almost in powder, so long as you can observe in course of digging it out that it is a single skeleton, and it can be bagged in a polythene container and kept in a state free from further contamination, it will probably be perfectly all right from our point of view. But if there is any danger that two or more skeletons have been mixed up, then the value of the material is decreased immensely.

Brothwell: Is cancellous tissue preferable even if it is already fragmented in the earth? If the samples were removed and put in bottles, would they be of use to you?

Smith: Yes, we can deal with that.

Ceppellini: I would suggest also taking a sample of the contaminating material, because this may be of the flora.

Smith: Bacterial antigens may also be present in the surrounding material.

Brothwell: Should it not be a normal part of the archaeologist's work, in the excavation of skeletons, to retain a soil sample? Nowadays much information can be obtained from its study, including data for the palaeoserologist.

Mourant: It has already been shown, in at least one published case that I recall, that the substances giving the reactions of blood group substances and indistinguishable from the apparent blood group substances in the bones were found in samples taken at the same time from the surrounding soil.

There is no doubt that until we know more about the best conditions for preserving this material, the ideal method is to preserve it at temperatures below 20° C. This has already been done for the quite irreplaceable and very well preserved material from Jericho.

Oakley: At the moment a deep-freeze is being installed in our Department at the British Museum (Natural History).

Siniscalco: Have these bones ever been X-rayed? This could give us some idea of the alterations that disease may have caused during life. For instance, the effect of thalassaemia major on infant skulls could be shown up quite sharply by X-ray examination.

Brothwell: Radiographic work would be quite expensive and may not be easily accessible to all archaeologists. As a preliminary measure in sorting, could not one measure the skull thickness? The foramen magnum gives one end of the calliper easy access to the endocranial surface.

Siniscalco: Yes, but alterations in thickness are not the only effects of disease on bones and it is just possible that X-rays would allow a satisfactory structural study of ancient bones without breaking them. This could apply particularly to the case of Cooley's anaemia.

Oakley: Have you published the X-rays of this condition?

Siniscalco: They have been published, but these are X-rays of *in vivo* material. Prof. Gatto in Palermo said that during the excavations at Messina he found some bones showing alterations which could be accounted for by thalassaemia major.

Lehmann: In a photograph of a skull with thalassaemia major one can see evidence for increased bone marrow function in the growing bone. One can see these skull alterations in thalassaemia major, because there is anaemia and, as a compensating mechanism, an increased red cell production in the growing bone; one can see it in syphilis, in sickle cell anaemia, in all types of anaemias due to infection, etc. Nevertheless, there is some very convincing evidence that certain spiculae and certain trabecular arrangements are rather more typical for thalassaemia major. One has to be very careful not to assume thalassaemia major just from a thickened skull.

Brothwell: Do you not also get a thickening of the shafts of the long bones?

Lehmann: Basically what occurs is an increased bone marrow function in the growing bone. The marrow expands, and the bones adapt themselves to it. The long bones can become very thick and they tend to become rectangular in shape. There is nothing specifically thalassaemic about it. You do get a certain change in the face—it has

been called mongoloid—but it has nothing to do with mongols and is due to the fact that certain facial bones thicken more and faster than others. It has even been claimed that certain facial features typically seen on mediaeval portraits in Ferrara were due to thalassaemia minor.

Siniscalco: I shall refer later (this symposium, p. 205) to the pleiotropic effect of thalassaemia on body measurements. I quite agree with you that neither can be taken as specific for thalassaemia. I was suggesting that X-ray measurements, since they are easy to perform and do not require breaking up of the bones, might prove to be a very useful means of investigation for ancient material and provide some information in addition to the usual osteometric data.

Barnett: What is the difference between Etruscan and contemporary Italian skulls? It seems that we have very few authenticated Etruscan skulls. Are there large numbers of Faliscan or other skulls from surrounding territories, which can be isolated?

Barnicot: We were able to find a number of modern Italian samples in the literature i.e. samples dated from the last few hundred years. There are no big series of ancient skulls in existence. We have collected records of about 200 Etruscan skulls. It is quite certain that many more do exist, perhaps in museums; and they may even be recorded in the literature.

Oakley: Would further metrical analysis be worth trying, in which you eliminate those skulls which are of dubious Etruscan origin? From our own collection, I would only rely on five of the ten which you measured. Would it not be interesting to repeat your analysis after excluding the items in the literature and in collections that you could not be sure about?

Barnicot: The literature that we have been able to look at gives no indication that could help. I do not think the experiment could be attempted.

Oakley: The sexing of skulls without associated pelves is largely a subjective matter. It seems that the overweighting in skulls identified as males might conceivably be due to an unusually high percentage of Etruscan females having a masculine cast of feature.

Barnicot: The main difficulty about sexing is that, since there are these systematic differences between the sexes, if you make a population comparison and due to bad sexing an appreciable proportion of female

skulls are included in a supposedly male sample, then of course you get into a muddle. It is very difficult to tell whether this has happened or not, unless one goes back to the original material.

Lehmann: Do people in Italy feel that the present-day inhabitants of Etruria are different from other Italians or not? In this country, for instance, people from the North think they are different from those of Wales.

Bolelli: The variations in the different Italian types are so marked that it is very difficult to give a complete answer to this. Generally we think there is a northern and a southern Italian type. The difference in shading is much more complex. We even see distinction between one region and another, e.g. Neapolitans and Sicilians. Italian history is so very complex; Italy is a country which has suffered so many different influences that it is not possible to identify a purely Italian type. We have as many types as we have Italian cities; each city has its own personality, its own soul. Those who know us will agree that it is very difficult to identify one single type.

Ceppellini: I don't agree, in one sense.

Bolelli: The dialects are equally varied; I do not think it can be said that there is one single Italian dialect.

Ward Perkins: As a foreigner who has spent a long time in Italy, I think there is an answer to that. There are many Italian dialects, but we are all aware when we hear one of those dialects that we are not listening to English. What has been said is very true, but that has also been said about every osteological measurement. One cannot deal with individuals, one must deal with bulk. I would have said that presented with a large crowd of persons in, let us say, Calabria, apart from the characteristics of architecture, I would guess that I am in southern Italy and not in the north. One can go even further than that. One will find many varieties in any place; but one does get an overall picture when one goes, for instance, up into the Abruzzi. One could eliminate certain types that have not been really characteristic of what comes into the generality.

Bolelli: To go back to the question of whether Italians feel that the present population of Tuscany constitutes the real descendants of the Etruscans, and whether the present population differs from other Italians: we do not even know what are the recognized characteristics

of the Etruscans, so how can we say whether or not they differ from other Italians?

We also spoke of the question of different dialects. Italy is a country with a very great linguistic variety, greater than in any other region of the world. In general, these can be said to be of a Latin type; but under this general heading we have an infinite variety of shades.

On the whole, the Italian population is so complex that it is very difficult to compare it with a non-Italian one. It is not easy to find a common denominator.

Ceppellini: One fundamental point which must be stressed is that we biologists do not mean that it is possible to decide whether a single individual comes from a specific region. Livi's index gives us different cephalic measurements for Sardinians and Tuscans, and it can be seen that the means differ significantly, whereas the means for Tuscany and Lazio are practically the same. But we can certainly define the distribution of different characteristics for different populations.

Bolelli: I had the impression that, in Livi's index, there was more variation in Lucca and in Pisa than in Florence or in Grosseto, but the linguistic data do not agree with this.

Morganti: Yes, but the differences are scarcely significant, and are due mainly to the effect of the size of the sample and to the methods of ascertainment. I shall show later (this symposium, p. 189) that the same applies to the blood groups.

Ceppellini: Prof. Bolelli is right, but we need the historians and glottologists to give us different kinds of subdivisions of the population now living, i.e. subdivisions in terms of history and glottology, not in terms of districts. Distribution should be studied in the light of the new classification of populations.

We should remember that we have at our disposal a series of data—anthropometric measurements and blood group estimations, for instance—which could be interpreted statistically. Our problem now is not to identify a single Etruscan type but to discover what was the general trend of distribution of those characters in the population of Etruria.

von Vacano: I would agree with Prof. Bolleli in that there is also no Etruscan type. We have 400–500 portraits from Volterra and it is not possible to say that they represent one single type; even if one takes

into account the fact that they have been studied so far only from an artistic point of view, and not by archaeologists in collaboration with biologists and anthropologists.

Ciotti: There is a fundamental objection to using a series of portraits and drawings from urns and sarcophagi for scientific investigations. Even the portraits of emperors on coins differ from one another; and these had the distinctive marks of the particular artistic schools. So how can we rely on these data for any scientific purpose?

von Vacano: I am not quite sure that the urn portraits from Volterra or Chiusi should be compared with portraits on coins or with any other idealized heads. It is generally acknowledged that their special characteristic is a strong realism. We know the peculiarities of the style of this era sufficiently well, and we have enough examples to enable a thorough examination to be made from the point of view of what should be ascribed to late Hellenism in Italy or to local artistic schools, and what to the anthropological type.

Certainly no attempt should be made to draw conclusions regarding type from every single representation of any one time; but a definite material, and one that because of its realism is particularly suitable, should be carefully examined by a team of archaeologists, biologists and anthropologists. Bearing in mind the scarcity of the sources at our disposal, such an effort seems to me not only justifiable but even advisable.

Banti: Take the coins, for instance: if you compare coins with photographs you will see very little resemblance between them. When I look at a British coin bearing the head of a woman, I cannot tell whether this is the head of Britannia or the Queen. This is because the artist who is asked to produce an official portrait produces an interpretation rather than a representation of his subject. The same might apply to Etruscan artists.

Riis: On the question of art, I share the opinion of some of my distinguished Italian colleagues that one has to be extremely careful in drawing conclusions from art. It is an established fact, and one on which most people agree, that early Italian art, Etruscan art, was submitted to different influences; for instance, in the 7th Century B.C. there was a certain Orientalizing influence, and facial and cranial features were made according to standards in the eastern Mediterranean regions.

Later there was an influx of Greek types from the Greek mainland; then the types were depicted in Italy according to standards on the Greek mainland. Still later there came influences from Ionia, then Attica, and then perhaps from the Greek provinces in Italy. Finally came the more naturalistic art. There it may be possible to find works of art where one could undertake measurements and use the data in comparison with the craniometric results that have been achieved on such material as Dr. Oakley's skulls. But one should not forget that, even in naturalistic art, style is an important factor; for instance, among the portraits of Augustus, there are Eastern and Western types. From those one could not possibly carry out craniometry. The archaeologists are familiar with these problems but the geneticists must not forget them.

Ceppellini: As regards blood group studies in bones, although some time has been spent on this problem, so far we have no real results. Therefore, before preparing a programme which might prove costly in time and money, we should be certain of being able to overcome the technical difficulties. For this, we need not only Etruscan bones, but other bones also. I have examined 40 crania found in the cellar of the Institute of Pathological Anatomy in Milan, and my results were unsatisfactory.

We should not neglect this problem; it must be tackled with all the scientific methods at our disposal, not only by the inhibition methods described by Mrs. Smith, but also, for instance, by the method of using fluorescent antibodies to see if they become attached to the trabeculae of the bones, and the method of producing specific sera by injecting suspensions of bone into rabbits. Here is a heavy programme of interesting work, which has nothing to do with archaeology or anthropology as such.

Ciotti: Well and good, but you must understand that just as we are forced to get rid of some of the archaeological pottery, so we are forced to get rid of most of the bones, which are not necessary for our own immediate studies. This brings us back to the problem of how to preserve skeletal material in its most useful form. So far, we have wasted this material, but from now on we can approach the question in another light.

Morganti: This reflects on the problem of blood groups in ancient

bones, but not on blood groups of living populations. But from now on we should no longer go our separate ways, because your advice could be most helpful for the problem of genetical analysis of living populations.

Siniscalco: A good solution would be to give the bones over to the biologists for storage in a deep-freeze. If the total number of skeletons found every year were of the order of 30–40, we would have no difficulty in storing them.

Ciotti: The number of skeletons excavated every year is not as low as 30–40, because quite apart from the Etruscan remains there are also the Roman ones. Some years we excavate a huge number.

Morganti: However, a comparison between Etruscan and Roman bones could prove most useful and interesting.

Ciotti: We should have an assistant biologist present at the site where excavation is going on, to advise us on what bones are worth preserving and how they should be preserved. The most useful part of the excavation would come from specific investigation and not from routine preservation of old bones. Therefore, we should carry out our excavations in close co-operation with the biologists.

Morganti: Perhaps we should decide to preserve only one type of long bone, e.g. the femur.

Foti: The long bones are usually found in better condition than the others.

Lehmann: If, from a large heap of bones which are difficult to disentangle, one were to take every right femur, and put it in a polythene bag, one would then have a sample of the large heap of bones taking up very little space.

Oakley: I agree with what Dr. Lehmann said, with the addition that a planned programme of research would be most beneficial. Something like a selection of either right or left heads of femora from Etruscan burial should be preserved in polythene bags, until we are ready to test them.* It would be a useful beginning. We could wait until excavation of a Roman necropolis provided material for comparison. Such research as we envisage cannot be done all at once.

Ward Perkins: Now is probably the best moment for this, because

* See appendix I, p. 246.

very large parts of Etruria are being ploughed for the first time and the material is coming up now in bulk. The types of sites I am thinking of come up every two or three weeks. As one goes around one finds a small cemetery which has been ploughed through; there are the bones. One can usually tell them by the context in which they come up, and they are mostly Roman. If that material is of any use (after all it is from Etruria, from a local population) now would be the moment to put some material by.

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THE USE OF GENETICAL CHARACTERS AS INDICES OF POPULATION DISTRIBUTION

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FROM the point of view of physical anthropologists, the inherited characteristics of the human body can broadly be divided into two classes, those which are external and directly visible, and those which are biochemical or physiological and need special tests for their diagnosis. The external characters, that is to say the shape, size and colour of the exterior of the body and its parts, are studied by direct measurement; and for almost any one of such characters a series of measurements, conducted upon a large number of persons, will show continuous variation. The genetics of these characters have been very little studied but it is certain that, with one or two possible exceptions, each of them is determined by the combined effects of a considerable number of genes. Moreover, the expression of these genes in any individual is considerably modified by the environmental history of that individual. Since other speakers in this symposium have discussed these quantitative characters, I propose to speak only of those of the other class, the qualitative characters which are determined by simple and fairly well understood genetical mechanisms, and chiefly of the blood groups, which are the most important characters in this class.

There are ten main blood group systems, each of which may be regarded as genetically independent of the others; the frequencies of the genes in nearly all of these systems have proved to be useful in the study of populations and their relationships with one another. A number of other genetically simple

characters, notably thalassaemia and the hereditary haemoglobin variants, have also been shown to have anthropological value.

It is not necessary here to describe in detail the methods of blood group testing, or even the genetics of the various blood group systems, but the following is a very brief explanation of the principles upon which the tests are based, and what the results mean biochemically and genetically. In order to ascertain the group of a blood specimen, the red cells of the blood are tested by mixing separate drops of a suspension of them each with a drop of a serum containing a specific antibody. If the red cells contain an antigen corresponding to the antibody in a particular serum they will clump together, or "agglutinate".

We may look upon each separate antigen as being determined by the presence of a particular gene in the constitution of the individual. The genes themselves fall into the ten genetically distinct classes or systems already mentioned. The one-to-one correspondence between antigens and genes, and the classification of the genes into separate systems, have been confirmed by extensive family studies. The genetical study of the blood groups is facilitated by the fact that, for nearly all the systems, a heterozygote, i.e. an individual who has received from each parent a different gene of a given allelomorphic set, has both the corresponding antigens clearly demonstrable on his or her red cells.

In the case of the *ABO* system the available tests can distinguish only four kinds of individual, or four phenotypes, each carrying a different set of two (like or unlike) of the three genes *A*, *B* and *O*; but the combinations *AA* and *AO* are indistinguishable by tests on the individual, as are *BB* and *BO*. When such an ambiguity is present, it is often possible to resolve it and determine the precise genotype of an individual by tests on other members of the family. Quite apart from family studies, however, it is always possible, by testing an adequate sample of a large interbreeding population, to calculate the frequencies of the genotypes, and of the individual genes, from those of the phenotypes. In

practice, the frequencies of the genes are calculated directly from those of the phenotypes, but the meaning of the concept "gene frequency" may be made clearer if we imagine that the frequencies of the various genotypes have been calculated first, or that the genotype of every individual has been ascertained; then by counting separately the two genes of each individual, the frequencies of the genes themselves may be deduced.

Special attention has been drawn to genes since, in population studies, it is convenient for many reasons to think in terms of genes rather than of phenotypes. One important reason is that (as can readily be shown mathematically) when two populations mix and interbreed there is no simple relationship between the phenotype frequencies in the original and in the final populations, whilst the genes themselves appear in the final population in the proportions in which they were originally contributed by the mixing populations. Another reason is that there are usually less kinds of genes than of phenotypes. It is, therefore, usual to plot blood group distribution maps in terms of gene frequencies.

The *Rh*, or Rhesus, blood group system comes second only to the *ABO* system in practical importance, both clinically and anthropologically. Genetically it is highly complex, but since many of the populations studied have been tested with one only of the various types of antiserum, anti-*D*, it is convenient to regard the system, as a first approximation, in terms of two groups or phenotypes, *Rh*-positive (or *D*-positive) and *Rh*-negative (or *D*-negative), determined by combinations of the allelomorphic genes *D* and *d*.

Regarding the complete genetical interpretation of the *Rh* system there is considerable controversy. The most generally accepted view is that a number of closely linked genes is involved. Fortunately, however, the use of the system in anthropology is little affected by the differences in ultimate genetical interpretation, i.e. whether we look upon a certain phenomenon as due to a single complex gene or to a number of closely linked genes.

Detailed *Rh* grouping, in which blood specimens are tested for numerous antigens (and hence genes) of the *Rh* system, is in fact being used more and more in anthropological work and has in many cases thrown light on population relationships. Any conclusions reached are independent of the precise genetical theory adopted, except perhaps where very long-term evolutionary questions are involved.

It is easy to show theoretically that where there is no natural or artificial selection for or against any of the phenotypes of a given blood group system, the gene frequencies of that system will remain constant from one generation to the next in any large closed population. The intensity of natural selection to which the blood groups are subject is almost certainly quite small, but there are some indications that the *ABO* blood groups are subject to rather more rapid selection than those of the other systems. It is, however, rather unlikely that, even in the case of the *ABO* groups, selective frequency changes over a period of 2,500 years will have been sufficiently great seriously to confuse the issues in a problem such as that of the possible relationship of the Etruscans to various modern populations.

The general approach in applying blood grouping methods to anthropological problems is to determine the gene frequencies on as large samples as possible, and for as many blood group systems as possible, for all the populations under study. If all the populations resemble one another with regard to the frequencies of one particular set of genes, this is by no means proof that these populations are closely related. If, however, the gene frequencies for a considerable number of genetically independent systems are similar in two populations, this is a strong indication that they are related to one another. On the other hand, a wide divergence in frequencies even for one system is rather strong evidence against close relationships, or at least suggests that if a relationship does exist the period of free interbreeding between the two populations has been followed by some important genetical event such as

intensive selection in one or both of them, a fall to very small numbers resulting in a random fluctuation of gene frequencies, or extensive mixing with an unrelated population.

Some concrete examples of the way in which the blood groups can be used in the study of anthropological problems are as follows. [Since Dr. Morganti (this symposium, p. 189) will talk about blood group distribution in Italy, I shall draw my examples from elsewhere.] One of the most surprising facts in the distribution of the *ABO* blood groups in western Europe is that the Icelanders have a very high *O* frequency similar to that of the Scots, Irish and Welsh and very different from the modern Scandinavians, who have some of the highest *A* frequencies and lowest *O* frequencies in Europe. The cause for this has by no means been finally settled; the short time which has elapsed since the settlement of Iceland makes it extremely unlikely that there has been any marked effect due to natural selection in any of the countries concerned. Two other main possibilities have been considered: one is that Iceland was mainly colonized from the British Isles and that the Scandinavians who are supposed to have colonized it, and who wrote the Sagas, were only a relatively small ruling class. More likely is the theory that the Vikings who went to Iceland had a high *O* frequency and were racially different from the modern Scandinavians, whose ancestors came in from the east and compelled the Vikings to take to a life of seafaring and colonization.

The highly detailed survey which is at present being done in Great Britain, based on the records of the National Blood Transfusion Service, is showing up some surprising local variations in blood group frequencies, some of which can already be correlated tentatively with historical events. It is hoped that, when the survey is completed, such correlations will prove to be very close indeed, though it is likely that a number of problems will be left which will need intensive local study. Incidentally, detailed studies in Great Britain considered in relation to known Viking

settlements may help to solve the problem of the Icelandic blood groups.

Among the peoples of northern and central Europe, apart from the Basques and the Lapps, the *ABO* blood groups are the only ones which have shown frequency differences between populations sufficiently great to enable them to be applied to anthropological problems. In the Mediterranean area, however, the genes of the other blood group systems vary considerably in frequency from place to place and it may be possible to apply them to a study of such matters as the origin of the Etruscans.

Before leaving the subject of blood groups, mention may be made of one new development which we have already discussed, namely, the study of the *ABO* blood groups of ancient human remains. There is no doubt that the *ABO* antigens are highly stable and can persist for thousands of years in bones and other tissues. The study of the blood groups of ancient tissues is still in its infancy and it may be months or years before work recently begun yields results of direct applicability to archaeological problems. It is hoped, however, that the new blood grouping laboratory at the British Museum (Natural History) will in a few years be able to make important contributions to the Etruscan and other problems.

To conclude with a few words about some other genetical systems: As an approach to the rather complex problems raised by the distribution of thalassaemia in Italy I shall speak of the much better defined situation that exists regarding the haemoglobins in Africa. There are two main haemoglobins present among Africans. By far the commoner is haemoglobin *A* (which is present also in nearly all Europeans); the other is haemoglobin *S*, or sickle-cell haemoglobin. These two haemoglobins are determined by a pair of allelomorphic genes the effect of which is that any individual who is homozygous, that is to say who has two genes for one particular haemoglobin, has only this haemoglobin in his or her red cells. Heterozygotes, however, with one

gene of each kind, have a mixture of the two haemoglobins. Something like 10 per cent of all Africans are heterozygotes for haemoglobins *A* and *S*. Most of the rest are homozygotes for *A* and a very small proportion are homozygotes for *S*. Nearly all the homozygotes for the *S* gene die in infancy of a severe haemolytic anaemia and thus fail to pass on the *S* gene to the next generation, and it was until recently a great problem to determine how a semilethal gene such as this could, nevertheless, persist with quite high frequencies in certain tribes. For instance, some tribes contain as many as 40 per cent of *AS* heterozygotes, from which simple calculations show that something like 5 per cent of all the births must be of *SS* homozygotes, nearly all of whom presumably die in infancy. It would thus be expected that the gene would progressively diminish in frequency and die out. The problem of its maintenance has been largely solved by the demonstration that *AS* heterozygotes are much more resistant to malignant tertian malaria than are normal persons. Thus, in conditions of intense malarial infection, the death rate of *SS* homozygotes from anaemia would be balanced by that of *AA* homozygotes from malaria, and the gene would persist.

The thalassaemia gene is prevalent throughout a large part of the Mediterranean area including much of Italy. Its manifestations are much more complex than those of the haemoglobins just mentioned, but here again heterozygotes are clinically almost normal whereas homozygotes for thalassaemia suffer from a very severe anaemia of which many die in childhood; very few of the homozygotes live to have children. The persistence of the gene with quite high frequencies, notably in the Ferrara region, presents a similar problem to that of sickling in Africa and elsewhere, and here again it has been suggested that heterozygotes must have some selective advantage over normal persons, perhaps again in their resistance to malaria. Owing to the probable existence of selective effects acting both for and against the presence of the thalassaemia gene, its frequency in a breeding

population may change with time, and its use in quantitative anthropological studies is therefore subject to certain limitations. A knowledge of the presence or absence of the gene in a given population is, however, of considerable value; and in studies of human ecology the probable existence of selective effects related to the environment is itself of great interest.

Thalassaemia is, moreover, a condition which is frequently recognizable by bone changes; specific examinations of human skeletons should be made for these changes. These are found particularly in the homozygous condition, thalassaemia major, but Dr. Lehmann says that they are found also in some cases of the heterozygous condition, thalassaemia minor. They are found too in the homozygotes for haemoglobin *S*, who suffer from sickle-cell anaemia, but such persons are very rare in Europe except in parts of Greece.

In this communication, I have not tried to reach any conclusions but merely to give some idea of the genetical method of approach to anthropological problems and to show that it has pointed to a solution of some such problems. I am not unduly optimistic that genetics will show us where the Etruscans came from, but the problem must surely ultimately be soluble, and it is certainly worth while applying genetical methods intensively to its study.

DISCUSSION

Cook: What exactly is a phenotype?

Mourant: If you take an individual and do a number of tests on that person's blood, then by simply considering the results of tests on that individual—just the combination of positive and negative results—you arrive at that individual's phenotype. That particular combination of observed results may be the effect of a number of different genetical constitutions or genotypes. In order to ascertain the genotype it may be necessary to test relatives as well.

Lehmann: It may also be described as follows: you may have a black cat; the phenotype of that cat is black, but if it carries genes for white and black, which it can transmit to its offspring but which in the present generation do not find expression in the phenotype (pheno— from the Greek word: to appear), then the genotype is black and white. Phenotype and genotype may not necessarily be the same.

We may discuss Dr. Mourant's address but it will be impossible to dispute anything Dr. Mourant has said about blood group distribution. He is virtually the Pope in this field!

Kirk: Dr. Barnicot suggested that the study of skulls shows that the ancient Etruscans were not different from other ancient Italian peoples. If the skulls are the same size as those of the rest of the population, are their genotypes necessarily the same? In other words, can there be blood group differences which have not manifested themselves in the actual physical appearance?

Mourant: Undoubtedly; and perhaps that is a good illustration for the difference between phenotype and genotype: because the genetics of skull shape and size are so complex, you may have a whole lot of different combinations of genes in different individuals and different populations, each giving rise to an indistinguishable skull type. All of the skull-shape determining genes are probably genetically independent of the blood group genes, just as the blood group genes are independent of one another.

From the point of view of genetics, blood groups are indicators of genes, just as hair colour is, and so on. The difference genetically between one blood group and another is no less than e.g. the difference between one blood group and hair colour.

Barnicot: What is the evidence that all the genes concerned with skeletal development are independent of blood groups?

Mourant: I think we shall find that some of them are linked; but I am almost certain that none of them are so closely linked as to affect the sort of statistics we are dealing with. As you know, two of the blood group systems, the Lewis and the Lutheran, are genetically linked, but the frequency of crossing-over is sufficiently large to allow the frequencies of the blood groups in the two genetical systems to be considered for anthropological purposes as completely independent.

Barnicot: I was thinking of a possible pleiotropism, rather than of linkage. Is that a possibility?

Mourant: It is a possibility, but it has not been tested.

Ceppellini: There is certainly a correlation in the offspring, between a given blood group, and e.g. *A* and *B* or *A* and *O*, and the shape of the skull. But it must be clearly understood that when we speak of different genetical systems, we mean that they behave in a completely independent way. Therefore, two populations may be completely different for one system, e.g. *ABO*, and absolutely identical for another system. As Dr. Mourant pointed out, usually it is quite sufficient for one genetical system to be different in order to assume that they are not closely related from the ethnic point of view.

Mourant: There is another point which has produced confusion in the past, when people have attempted to show that there is correlation. If pressed, they would probably have called what they found "pleiotropic effects" of blood group genes. But nearly all the studies that have so far been done have not been properly designed statistically, and the correlations obtained were invalid. For instance—to take a hypothetical extreme case—if you mix a Negro population, with a high frequency of blood group *O*, and a white population, with a very high frequency of group *A*, and if complete genetical mixing occurs, then there will be no correlation between the blood groups and, for instance, the colour of the skin. But if mixing is incomplete, then there will be a very high correlation between skin colour and blood groups. People have failed to realize that what they have seen was really correlation in two populations which had not completely mixed genetically.

Siniscalco: In that case there is only a tendency to correlation within the population, but not within families.

Mourant: Yes.

Bullough: You said that a more detailed map of Britain would show variation, and it would have to be more refined than 5 per cent. Is this statistically significant?

Mourant: On the numbers we have tested it is very significant. We have already obtained results for 350,000 people in Britain, so that variations down to 1 per cent may be quite significant in the type of survey which we are making.

Bullough: Is there any calculable statistical relationship in the distribution? For instance, in the case of the cat which is black, but where the genotype is black and white: can you say that if the cat has six kittens there should be a certain distribution, and what are the unknown factors?

Mourant: If there are six kittens you can say what the distribution is likely to be. But if you mate e.g. 1000 homozygous white cats with 1000 heterozygous black and white cats, you can predict fairly accurately how many kittens of each colour will be in the offspring.

Bullough: In this case, I should have thought that we are looking at the results and trying to determine population links in historic times. But, in fact, we do not know at any point the number of starting points which we have, and we are taking an overall picture. You have admitted that where you have had at one time a sharp reduction in a population, the predominance of one or other blood group later may be the result of some person's abnormal fertility. I think there is a factor of real importance here that may upset the results.

Mourant: When I said that differences of one per cent in different parts of Great Britain are significant statistically, I was merely saying that this is a point which can be determined by the statistician: that a difference of 10 per cent or of 1 per cent may or may not be statistically significant. For instance, almost independently of genetics, if you have two populations and you only take one hundred of each and you get a 10 per cent difference, that is not significant; the original populations may have been identical. But if you have really big populations and you push your sampling far enough, then you may reach a point where a difference of 1 : 1000 is statistically significant, but it is not anthropologically significant.

Bullough: The difference between a statistically and an anthropologically significant result is clearly important for historical purposes. Take, for instance, Iceland and Scandinavia: the question of linking statistically significant results in one place with statistical results in another, and then saying that this has some anthropological significance, makes an assumption about the relative size of the population at the starting point which is probably unjustified. In particular, if the population migrated on ships to Iceland, you obviously start with a limited number of people who may well be of one particular kind.

Mourant: That is so.

Bullough: That seems to me to upset somewhat the value of the medical evidence for the historian; unless you have an idea of the number of persons involved at the starting point.

Mourant: This is one of the things we are trying to find out.

Kirk: This is presumably the reason why Prof. Ceppellini wanted to know what was the population of ancient Etruria.

Bullough: The historians can never provide statistical material comparable to modern statistical material. Where articles are written on size of the population, size of the family, expectation of life, etc., in fact statistical methods are being applied to very crude material, because there has already been the artificial sampling by survival. This bears no relationship to the statistical evidence eliminated by statistical methods. For instance, you cannot work out statistical data when you are dealing with inscriptions which happen to have turned up in one cemetery.

Kirk: It depends on what the scientists require. Is it valuable for them to know, e.g., that the total population of ancient Etruria lay perhaps not lower than 50,000 and not higher than 200,000? The archaeologist and historian may be able to commit themselves to such broad limits as that.

Mourant: I should think that would be very helpful.

Ward Perkins: The archaeologist or historian can provide that. All these are approximations; even the results obtained from mediaeval charters are approximations in those cases where an element of judgment comes in. In historical work we are not dealing with an exact science, but with evaluation of a number of variables, out of which we attempt to get something which is as close to the truth as possible.

When one gets a statistical figure, one does not sit down on 100 results; one goes to other places and sees if it does work. If it works, perhaps the hypothesis seems a bit better; then one goes on testing it. We must be prepared for jokers in the pack. But I think that to throw in one's hand on the grounds that one cannot get certitude is simply to throw away one's results in any archaeological or historical work.

Bullough: I do not agree. This is a serious confusion of the different nature of the methods used.

Bolelli: I do not want to minimize the importance of the research on human measurements which has been discussed at this symposium. and which is of the greatest interest. I would, however, comment that the biological maps seem to give us information which differs completely from what the historians believe to be true. It is clear that biological maps showing distribution of blood groups and metrical measurements disagree entirely with the maps showing the results of historical research in a given area. The problem of Iceland, for instance, is very interesting: according to the biological data, Iceland is quite separate from the Scandinavian countries, which would seem to us historically contradictory.

These are important points and must be borne in mind when we plan to combine our data with yours. Reality has many facets, and perhaps your point of view and ours cannot be reconciled. You are probably right, and so are we, but to combine our separate approaches does not seem to be feasible.

Ceppellini: I believe that these difficulties arise only because we are at the very beginning in this field. At any rate, it is of prime importance to compare biological maps with your cultural and historical ones, in order to study the correlation between cultural, historical and biological populational entities.

Morganti: The main reason for a contradiction between the two sets of data regarding Iceland might be explained by a hypothesis that only very few individuals from Scandinavian regions actually went as a ruling class. This could explain why cultural elements are present and biological elements are absent. This illustrates how much we biologists need the help of the historians. It is up to you to tell us about the number of people who actually migrated from one place to another; e.g. if you were to tell us that 50,000 and not 50 people migrated from

Scandinavia to Iceland, then it would be true to speak of a contradiction between the two sets of data.

Bloch: Prof. Bolelli's remarks would apply also to the difficulties which arise when we compare archaeological and linguistic maps. Of course, this does not mean that we should despair of correlating the two sets of data. One might quote Pallottino's book (1947, L'origine degli Etruschi. Rome: Tumminelli) where he attempted to correlate the linguistic with the archaeological data. Pallottino also found this a difficult task, and the results are not straightforward.

Siniscalco: There is no doubt that the difficulties that Prof. Bolelli has pointed out do exist in reality. When we are dealing with the problem of tracing back the racial history of man we have different ways of approach. "Reality" has indeed many facets, but the point is that not one of these facets can stand alone: we must combine them in order to obtain a better estimate of "reality".

Here is an example: as Dr. Morganti pointed out, an immigration might have an effect on the genetical structure of a population only if the number of immigrants was relatively appreciable. Then, if it is established by historical and archaeological findings that an immigration has taken place into a particular area, but information about the actual number of people is lacking, the finding of a biological heterogeneity in that area, as compared with the surrounding ones, would permit us to obtain a rough estimate of the number of individuals who immigrated.

Oakley: Darlington has published interesting work on correlating distribution of linguistic forms, i.e. the ability or inability to pronounce *th*, with the incidence of blood group O in populations (Darlington, C. D. (1947). *Heredity*, **1**, 269).

Mourant: There may be something in it; but I think that Darlington has carried it too far. It is a problem worth investigating, but as yet there is very little proof. When the work was published there appeared to be some striking correlations, but a number of maps which have come out since Darlington published his papers do not agree well with his theories.

I cannot speak personally for any correlation between historical or linguistic maps and genetical maps. But it is rather striking that in those countries where detailed blood group surveys and detailed

anthropometric surveys have been done, e.g. in Ireland, there is a rather close correlation between the lines of frequency of the different types of blood group genes on the one hand, and skull types on the other.

Barnicot: Darlington's work on the *th* sound reminds one that in all this work where one attempts to correlate cultural, linguistic and biological features, one has to be exceedingly careful in interpreting the causal basis of these correlations. I think the great criticism of Darlington's particular contribution was that he supposed that one was dealing with some genetical conformation of the tongue or the palate, or something that made it impossible for some people to pronounce *th*. It was that particular point that failed to win acceptance. He gave certain instances of supposed physical differences of an inherited nature which might affect pronunciation; not all geneticists would agree upon these. It was his interpretation of the causal connexions of the observed linguistic correlation that was probably most open to objection.

Oakley: In that connexion, I am very interested in what Prof. Bolelli said about the inheritance or cultural passing on of habits of speech which could be so easily misinterpreted as being due to differences in the organs of speech. I thought that this was a point which might elucidate Darlington's findings.

Ceppellini: To convince our colleagues, instead of the work of Darlington we could mention the results of the blood groups in England and the boundary between England and Scotland; there is some kind of correlation.

Mourant: I must be very careful, because I think that the blood group boundary comes right down into Northumberland or Durham—we really do not want to give those counties to Scotland!

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BLOOD GROUPS AND HAEMATOLOGICAL DATA AS A SOURCE OF ETHNIC INFORMATION

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THE use of biological markers as a method of ethnological investigation has certain advantages and limitations, and these may be clearly illustrated by the results of our studies of some populations in Italy.

In the biological approach to ethnology, the blood groups form the most important of the genetical systems, but thalassaemia has also been considered because of its peculiar geographical distribution. However, there is a great difference between thalassaemia and other haemoglobinopathies on the one hand, and between it and blood groups on the other. Blood groups are a physiological trait, without any implication for the health of the individual or for his probability of survival; whereas in the case of thalassaemia different genotypes have quite different probabilities of survival. Thalassaemia is inherited as a simple Mendelian trait, i.e. it depends on a single gene difference. Since man is a diploid, two genes give rise to three genotypes, the normal, the homozygous thalassaemic and the heterozygous genotype. In the heterozygous genotype the two allelelomorphic genes are different. The homozygous genotype is subject to very severe anaemia and usually dies in early childhood, very rarely reaching the reproductive age. In contrast, the heterozygous genotype may be recognized by certain peculiarities in the blood picture but enjoys good health and, under normal conditions, has the same probability of survival as the normal genotype.

Thalassaemia is widespread throughout the world but occurs chiefly in the Mediterranean area and is, therefore, also known as Mediterranean anaemia. In this area, however, the frequency of distribution varies greatly from one population to another. It is relatively frequent in North Africa, Greece and Cyprus and, in Italy, in the district of Ferrara and nearby Rovigo, in Sardinia and, to a lesser extent, in some regions of Sicily, Calabria and Puglia. The pattern of distribution of the disease has given rise to the theory, supported by many authors, that thalassaemia was imported into Italy by an immigrant population, perhaps Greek, Phoenician or Etruscan; but this theory does not help to solve the problem of how such an unfavourable gene could persist in certain populations at such a high frequency. In some villages in Ferrara and Sardinia there is a 20 per cent incidence of heterozygous thalassaemia; in each generation a certain percentage of the children will be homozygous and therefore eliminated, so that within a short period of biological time—say 10–15 generations—the disease should disappear. How then can we explain the fact that a 20 per cent incidence persists in these villages? The implication of the migratory theory is that these—and only these—populations must be steadily reinforced even at the present day by a number of Greeks, Phoenicians or Etruscans to replace the lost genes, which is absurd. Thalassaemia, therefore, must be an example of so-called balanced polymorphism, where the loss of genes is compensated: i.e. because of some special environmental conditions thalassaemic individuals have some kind of advantage over normal ones; it may be that they attain the reproductive age more readily, or that they have a greater number of children, and because they transmit one thalassaemic gene to the next generation they compensate for the loss of this gene due to the elimination of homozygous individuals. If it is true that the balance is maintained by a special environmental condition, then one outstanding environmental factor must be common to the different thalassaemic areas; and there is reason to believe that

this common factor could be represented by malaria, especially since thalassaemia has a high incidence only in malarial regions; a similar relationship between the incidence of sickle cell anaemia and malaria has already been established.

In order to test our hypothesis we selected for study four isolated villages in Sardinia where, according to historical, geographical and linguistic data, the populations were not likely to differ in ethnic origin. Two of the villages, Orosei and Galtelli, were situated in a most desolate valley between Nuoro and Urbia, where there was until a few years ago a very high incidence of malaria; the other two villages, Desulo and Tonara, which served as a basis for comparison, were situated high enough on the hills to be in a malaria-free zone. The frequency of thalassaemia carriers was estimated by means of a simple haematological test, and it was found that in both the villages in the malarial valley region the frequency is high (18·83–21·28 per cent) whereas in the malaria-free mountain zone it is relatively very low for Sardinia (Table I). This suggested a relationship between the environment and the frequency of the genetical traits under study, but it remained to be proved that the two groups of populations—mountain and valley—were homogeneous from the ethnic point of view. The dialects were not studied but were assumed to be Sardinian.

As independent genetical markers, *ABO*, *MN*, and *Rh* blood groups were used. When the *ABO* blood group frequency in the four villages was investigated and interpreted statistically, the distribution for the mountain populations was found to differ significantly from that for the valley; but on comparison of the two mountain villages with one another these were also found to differ significantly, and the same held true for the pair of villages in the valley (Table II). So it cannot be concluded that the mountain and valley populations differ, but rather that each of the four villages represents a genetical unit, a distinct population, and does not provide data on ethnic origins. As regards the *Rh*

Table I

DISTRIBUTION IN THE FOUR VILLAGES OF INDIVIDUALS SHOWING A MARKED DECREASE OF RED CELL OSMOTIC FRAGILITY

(Positive: less than 50 per cent in 0·4 Siemmel solution—confirmed by other haematological tests this criterion is consistent with the diagnosis of thalassaemia minor.)

		Inhabitants	*Tested*	*Positive* n per cent	χ^2	d.f.	P
Valley (malaria pandemic)	Orosei	4,850	308	58 (18·83)			
					0·50[1]	1	0·50–0·30
	4·5 km.						
	Galtelli	2,526	235	50 (21·28)			
50 km.					46·94[3]	1	<0·001
	Desulo	3,553	320	12 (3·75)			
Mountains (no malaria)							
	8 km.				0·18[2]	1	0·70–0·50
	Tonara	3,025	107	5 (4·67)			

For footnotes, see Table II.

Table II*

DISTRIBUTION OF A_1A_2BO GROUPS IN THE FOUR VILLAGES

	Tested	O	A_1	A_2	† B	† AB	χ^2	d.f.	P
Orosei	308	163 (52·92)	64 (20·78)	27 (8·77)	44 (14·29)	10 (3·25)			0·01–
							12·77[1]	3	0·001
Galtelli	235	117 (49·79)	70 (29·79)	27 (11·49)	16 (6·80)	5 (2·13)			
							29·18[3]	3	<0·001
Desulo	320	141 (44·06)	145 (45·31)	15 (4·69)	12 (3·75)	7 (2·19)			
							40·01[2]	3	<0·001
Tonara	107	57 (53·27)	20 (18·69)	22 (20·56)	6 (5·61)	2 (1·87)			

* Tables I–V are taken from Ceppellini, R. (1955). *Cold Spr. Harb. Symp. quant. Biol.*, **20**, 252. Reproduced by permission of the Editors.

† For χ^2, *B* and *AB* summed up.

[1] Between Orosei and Galtelli.

[2] Between Desulo and Tonara.

[3] Between Valley and Mountains.

blood group frequency, no significant difference was found either between the mountain and the valley populations or within each pair of villages; but in all four villages an extremely low frequency (2·60–5·94 per cent) of *Rh* negative was observed (Table III).

Table III

DISTRIBUTION OF *Rh* TYPES IN THE FOUR VILLAGES

	Tested	*Rh* positive	*Rh* negative	χ^2	d.f.	P
Orosei	308	300 (97·40)	8 (2·60)			
				1·14[1]	1	0·30–0·20
Galtelli	235	225 (95·74)	10 (4·26)			
				3·07[3]	1	0·10–0·05
Desulo	320	301 (94·06)	10 (5·94)			
				0·24[2]	1	0·70–0·50
Tonara	107	102 (95·33)	5 (4·67)			

For footnotes, see Table II.

This is important, because in Europe such low *Rh* negative values are found only in Sardinia; thus, it is clear that the four villages are typically Sardinian. The same result was obtained for the *MN* blood group frequencies; no significant difference between the mountain and valley populations or within each pair of villages, and an extremely low incidence of the *N* gene which is typical of Sardinia and not found anywhere else in Europe (Table IV). From these data it may be concluded that the difference in frequency of thalassaemia is not due to ethnic heterogeneity but to a special environmental selective factor;

Table IV

DISTRIBUTION OF *MN* TYPES IN THE FOUR VILLAGES

	Tested	*M*	*MN*	*N*	χ^2	d.f.	P
Orosei	308	198 (64·29)	91 (29·54)	19 (6·17)			
					1·11	2[1]	0·70–0·50
Galtelli	235	141 (60·00)	79 (33·62)	15 (6·38)			
					5·02	2[2]	0·10–0·05
Desulo	320	187 (58·44)	114 (35·62)	19 (5·94)			
					3·58	2[3]	0·20–0·10
Tonara	107	52 (48·60)	49 (45·79)	6 (5·61)			

For footnotes, see Table II.

it cannot be assumed that the populations of the valley are descended from immigrants and only those of the mountain are descended from ancient Sardinians.

It is of interest to compare the thalassaemic populations of these Sardinian villages with the thalassaemic district of Ferrara and, for comparison, with Milan. Because of the variation in frequency of the *ABO* blood group from one village to another, this system does not provide much useful data, but in general it indicates that there is a higher frequency of the *O* gene in Ferrara and in Sardinia than in Milan. The frequency of the *Rh* negative blood group in Ferrara is higher than in Sardinia but much lower than in Milan (Table V). This is of particular importance because Ferrara, in the north, forms a small pocket which is surrounded by populations which have a high *Rh* negative frequency. It seems, then, that Ferrara and Sardinia have something in common with one another, but again such a conclusion is disproved by the data for the *MN* blood group: whereas Sardinia has an extremely low *MN* frequency, Ferrara and Milan have the *MN* frequencies which are typical of Europe; and the difference in value for one single blood group system is enough to rule out the possibility of a common origin for the Sardinian and Ferrarese populations, despite the similarities in value for thalassaemia, *ABO* and *Rh* frequencies.

Table V

GENE FREQUENCIES FOR A^1, A^2, *B*, *O*, *Rh* positive (R), *Rh* negative (r), and *MN*, IN THE FOUR SARDINIAN VILLAGES, IN THE THALASSAEMIC REGION OF FERRARA (PO VALLEY) AND, FOR COMPARISON, IN MILAN

	Orosei	*Galtelli*	*Desulo*	*Tonara*	*Ferrara*	*Milan*
A_1	12·3	17·1	27·3	10·5	18·2	24·8
A_2	5·8	7·7	3·4	11·2	3·5	4·1
B	9·2	4·7	2·8	3·8	5·8	5·8
O	72·3	70·5	66·5	74·5	72·5	65·3
M	79·1	76·8	76·3	72·0	58·6	59·2
N	20·9	23·2	23·7	28·0	41·4	40·8
R	83·9	79·4	75·6	78·4	71·2	60·3
r	16·1	20·6	24·4	21·6	28·8	39·7

Let us suppose that Ferrara and Sardinia resemble one another in some respects, not because they have a common ethnic origin but because they are subject to similar environmental conditions. We might then assume that, in this particular case, an external environmental factor, e.g. malaria, or an internal one, e.g. thalassaemia, has been selective for the *ABO* or the *Rh* blood groups. This conclusion is not in agreement with the widely held belief that blood groups are free from environmental influence. Actually, it should be said that they are only relatively free; in other words, if they are influenced by an environmental factor this can take place only over a long period of time, i.e. thousands of years. On the contrary, traits with a high adaptive value, e.g. thalassaemia, are much more rapidly influenced by environment. Thus, it may be hoped that blood groups will prove to be of considerable value in ethnological research, while the use of pathological traits as biological markers would only be confusing.

It should be emphasized that in our own studies we are not dealing with large regions, but with villages, which are more meaningful from the biological point of view since they represent the true Mendelian populations, a group of people who share a common pool of genes; and this may be the approach which is needed for investigating the problem of ancient populations in Italy. The anthropologists, historians and the ethnologists must try to tell us what are the boundaries of a given ethnic group, in the present case what are to be considered as the Etruscan and non-Etruscan areas. We, in our turn, can investigate quite objectively the present-day populations within these boundaries. Whether the results are positive or negative the task is worth undertaking: the data are needed not alone for the study of ethnic origins, but also for a better understanding of the distribution of human blood groups.

DISCUSSION

Siniscalco: After this beautiful paper which has brightened the hopes of the archaeologists and ethnologists, I must throw a little cold water on the fire by emphasizing the difficulties which are met with in this kind of investigation and which may affect the interpretation of the data. In the case of Prof. Ceppellini's data, nothing prevents us from thinking that, in practice, some kind of differential adaptations have taken place both as regards *MN* groups and thalassaemia. We might assume that Sardinians have typical *MN* distribution because of selective adaptation. It must be so unless we really think that there are grounds for believing that Sardinians represent an ethnic strain which is completely different from all other European populations. At the same time we assume that the same environment, because of malaria, might have influenced the distribution of thalassaemia. If this were the case, one could infer that groups of individuals of the same ethnic origin and carrying the thalassaemic gene, who migrated at the same time to Ferrara and to Sardinia, might have been selected only for thalassaemia in Ferrara, and for thalassaemia and the *MN* blood group in Sardinia. Therefore, the difference found nowadays does not necessarily rule out the possibility of a common ethnic origin, provided that the selective pressure on the *MN* system is such that an effect of this kind could be obtained by natural selection over a period of 1000 years or so.

I must make clear that with this criticism I do not underestimate the value of this research, but emphasize that it is very difficult to draw any firm conclusion, on anthropological grounds, when we are dealing with characteristics whose adaptability to the environment is not well known.

Ceppellini: I refute Dr. Siniscalco's objection! Generally speaking, what he says is right. But it seems to me absurd to compare the adaptability of *MN* with that of thalassaemia; it is far more likely that thalassaemia would vary, and the group *MN* would have been maintained unchanged. Of course, you can turn the situation upside down, but as far as we know at present, the *MN* group is a standard as regards ethnic origins, whereas thalassaemia is a variable factor.

Siniscalco: I did not mean to turn the situation upside down! I

merely said that both characters might be adaptive (as, in fact, all characters are, in the long run); and indeed thalassaemia more so than the *MN* group. This would simply mean that to achieve the present situation only a relatively small number of generations was required, as far as thalassaemia is concerned, and many more were required for the *MN* group. It seems to me that it is easier to admit that the *MN* group distribution in Sardinia is, for some reason, the result of selection, rather than to assume that Sardinians—because of their *MN* gene frequencies—must be considered a peculiar race, completely different from all other European populations.

Ceppellini: It is clear that both the Sardinians and the people of Ferrara derived from Adam and Eve. The fundamental point is the following: a separation of the two ethnic groups—if they had a common origin, as they probably had—had taken place a long time ago, long before the time in which we are interested at present.

Siniscalco: This does not seem to be the case here. Actually the difference in gene frequencies for the *MN* system between Ferrara and Sardinia is of the order of nearly 10 per cent. Therefore, it is not inconceivable that such difference—if it is a significant one—might have arisen through a differential rate of adaptiveness in the two localities within the time limits of thirty generations or so. I feel, therefore, that before drawing any conclusion from Ceppellini's data something more should be known about the possible adaptive value of both genetical systems.

Morganti: Although in discussion generally we stress the points on which we differ rather than those on which we agree, in fact we are basically in agreement. But I am more in agreement with Ceppellini, because one may think that a strongly adaptive genetical character like thalassaemia might easily have been diffused in Ferrara, even if only very few people actually immigrated there; whereas the *MN* gene was unchanged because the *MN* gene is less adaptive. In Sardinia perhaps there were more immigrants and so the situation may be different. Therefore, the conclusions drawn by Ceppellini are quite tenable.

Siniscalco: I would like to make clear again that with my criticism I did not mean that the conclusions drawn by Ceppellini are untenable; on the contrary, they seem to me very suggestive but unfortunately

they cannot be taken as very probable because the data might, as I remarked, be interpreted in the opposite way. There is no doubt, however, that this paper has been an excellent illustration of how genetical information can be used in ethnological studies.

Lehmann: Cipriani has published that some features of Sardinian skulls—in the nasal spicula, the foramen magnum, the shape of the forehead etc.—reminded him of those seen in Veddas (Cipriani, L. (1934). *L'Universo*, **15**, 923; (1934). *Int. Congr. Anthrop. ethnol. Sci.*, p. 143; and personal communication). Veddas are aborigines of Ceylon with particularly remarkable skulls, and similar skulls can be found amongst the aborigines of India and in Arabia. There are also some prehistoric skulls with Veddoid features, notably in pre-Dynastic Egypt and in the Grimaldi cave in Monaco. Now we hear that there are some remarkable blood group frequencies in Sardinians which are equalled only—as far as I know—in some aborigines of South India. This recalls to my mind Cipriani's observation.

A point which is more relevant to the present discussion is the value or non-value of pathological genes for anthropological classification. I absolutely agree with everything Prof. Ceppellini said. If genes are very pathological, we can only use them superficially as markers. If thalassaemia exists in a population, it suggests that at some time this gene has been imported. It may have been imported by independent mutation, in which case it is of no significance anthropologically, or it may have been imported by immigrants. If these immigrants were few they might nevertheless be of great anthropological interest. Cultural connexions, for instance, can be established by a very small number of people. It is worth while to remember that thalassaemia is a misnomer; there are far more people with the thalassaemia gene in India, Burma, Malaya and China, than there are in the Mediterranean region.

Finally, I think we have yet to introduce another aspect. Take the sickle cell gene, which we now know has been influenced in its frequency by malaria. Surely, when malaria is eradicated, then the sickle cell gene when it has reached a certain equilibrium will become of far greater anthropological value than it is now. It will go down to a certain frequency where it is highly unlikely that homozygotes will arise; it will be fairly stable at a low frequency.

Ceppellini: In other words, in 200 years time for example, it may be concluded that Greek Cypriots have come from the Congo, because they have 20 per cent sickle cell there; and if malaria is eradicated, then we will have 2 per cent sickle cell as in the Congo, because malaria in the Congo will also be eradicated.

Lehmann: Then you will be able to say that this gene, if it has not arisen by independent mutation, indicates some connexion between Greece and Africa.

Ceppellini: Yes, one possible connexion: that 100 years ago or earlier, malaria was present in the Congo and in Cyprus.

Mourant: And that probably one African gene got into Greece.

Lehmann: We know that genes at one time in history may be of considerable positive selective value, in which case the anthropological interest is not so great. At other times they may have been neutral and yet at other times they may have been disadvantageous. A good example is the gene for diabetes. We know that diabetes in these days, because of insulin treatment, is much less dangerous than it used to be. Diabetics are therefore increasing in number. It has also been reported that people with a tendency to diabetes seem to be able to have more children—women with the diabetic gene, if properly treated, seem to have more children than other women (Aschner, B. M., and Post, R. H. (1956–57). *Acta genet.* (*Basel*), **6**, 362). So it seems that the diabetic gene has a positive selective value which is divorced from its deleterious effect of causing diabetes. At a time when there were no cereals eaten and people lived automatically on an antidiabetic diet, as in prehistoric, pre-agricultural times when cereals formed none or only a part of the daily diet, the advantageous effects of this gene must have come to the fore, and that might explain why nowadays diabetes is widespread in so many populations.

We must consider not only the present advantage or disadvantage, but also possible changes of value in the history of mankind.

Barnicot: The suggestion that there is any connexion whatsoever, as judged by skull measurements, between the Veddas and the Sardinians cannot be taken seriously. The only connexion, as far as I know, is that they both have rather long heads. Anyone who has seen a picture of the Veddas, with their rather broad noses, projecting faces and somewhat negroid appearance, and who has any acquaintance

with Sardinians can see immediately that they are not in the least like one another.

Brothwell: Can the serologist tell us whether the blood group frequencies, especially of the *ABO* group, might have displayed a greater rate of change in the past. It is evident that at least some of the commoner diseases, when they were first introduced into a population, were far more virulent and could therefore have been a powerful selective force.

Mourant: That is very likely. This applies to gene frequencies in late Palaeolithic times, at the end of the Ice Age, when man was expanding into new environments and was exposed to new diseases, new food, etc.

Brothwell: Presumably new diseases were still being introduced even during the Etruscan period, for instance leprosy.

Mourant: Malignant tertian malaria is a comparatively new introduction into most parts of the world where it exists. That is the one from which sickling gives protection. Is there any indication from Italian statistics or any other statistics that one particular form of malaria is protected against by thalassaemia?

Ceppellini: Plasmodium falciparum, for instance, is always present in Italy.

Lawler: Prof. Ceppellini, can you tell us what antimalarial measures were practised, particularly in these two villages? There would seem to be a splendid opportunity for showing what happens when a population is subjected to such outside influences.

Ceppellini: Your suggestion is very opportune, because we are living in a very important time. Some opportunities are just coming up now. Malaria was eradicated in Sardinia in a very dramatic way between 1946 and 1948. Of course the dynamics have changed, but probably also before that. At the end of the last century the importance of malaria was already agreed upon. Therefore, as was suggested by Silvestroni, Montalenti and Siniscalco, it may be possible that we are no longer in a phase of equilibrium, as far as thalassaemia is concerned. Thalassaemia was already decreasing before the appearance of malaria.

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DISTRIBUTION OF BLOOD GROUPS IN ITALY

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OUR knowledge of the distribution of blood groups in Italy is still imprecise and incomplete, particularly in regard to the populations of Tuscany, Umbria and Lazio, which would be of greater interest to us than those of the neighbouring regions. In this paper, an attempt is made to effect a synthesis of the data so far published in the literature (for references, see Morganti, 1957; Mourant, 1954). A systematic investigation of the existing data which have not yet been published has been undertaken by the National Committee for the Study of Blood Groups in Italy, formed in 1957 under the patronage of the High Commissioner for Hygiene and Public Health; and the findings relative to the *ABO* system will shortly be made available.

Instead of presenting the numerical data in tabular form, I have incorporated them graphically in geographical maps. This method is open to the criticism that often a choice has to be made between contradictory data. What should be accepted and what rejected—by what criteria should they be judged? Such a critical selection should, in fact, take into account at the same time the scientific stature of the author, the number of cases investigated in each group, the period in which the research was carried out, the consistency of the data, the source of the material, the methods of sampling employed, and other factors not always easily assessed.

Another problem is that of the choice of a territorial unit. As far as the evaluation of the kind of data so far available is concerned, I believe that, in general, the Province is the unit most suitable

for the purpose, for two fundamental reasons; firstly, the chief town is usually the only important urban centre in the area and, as such, has exerted for centuries and still exerts a sufficiently steady attraction on the flow of immigrants from the surrounding territory such that its population constitutes a representative sample of them; secondly, because the population of the urban centre is usually sufficiently comprehensive and intermixed such that a random sample can reasonably be regarded as adequate. This, of course, does not hold true for centres like Rome or Milan which attract immigrants from far beyond the confines of their own region; and neither does it hold true where there exist in the Province, besides the chief town, other important urban centres or consistent ethnic nuclei which, for historical, anthropological or glottological reasons, can properly be considered separately.

It must also be borne in mind that the greater part of the data at our disposal were not collected for the purpose of archaeological research but rather as a "by-product" of determinations made for some other reason (on blood donors, military recruits, pregnant women, etc.). Consequently, for some more important centres, a great deal of data has been collected by various authors, while for other populations we have no data whatever or only collective data grouped according to regions. Naturally, in the case of research carried out purely from the anthropological point of view, the criteria of sampling and grouping should be more appropriate.

In compiling our maps, we have subdivided the frequencies observed for each gene into a limited number of classes, each of which corresponds to a particular type of shading on the map. In deciding on the number of classes, we had to take into account, as always, two contrasting requirements: if a large number of strictly limited classes were used, this would lead to emphasizing barely significant differences; while the use of too few and too large classes would lead to distortion of the facts. However, in

the graphic representation of values for two contiguous classes, a marked difference may appear to exist between the upper and lower limits of those classes which, in reality, is appreciably less than the difference between the upper and lower limits of one single class.

The graphic representation of the gene frequencies of the *ABO* system constitutes a small problem in itself. The ideal method, of course, would be one which would permit the simultaneous representation of the variations in frequency of the three genes. Various methods have been suggested and used, but even the best ones cannot be relied upon to give a quick and comprehensive synthesis of the phenomena represented. Therefore, I have considered the frequencies of the three genes separately.

The frequencies of distribution of the $A(p)$ gene (i.e. the frequency of distribution, p, of the gene, A) (Fig. 1) are highest (0·31–0·35) in Liguria and at Busto Arsizio in the district of Varese, and lowest (0·16–0·20) in northern Sardinia and in the Mandrogni in the district of Alessandria. In the remainder of the population there is a rather even distribution, with a predominance of slightly lower values (0·21–0·26) in the south, excepting Veneto and the prealpine zone of Lombardy, and slightly higher values (0·26–0·30) in the rest of the country. It is noteworthy that the highest and the lowest values for the A gene correspond to ethnic nuclei which are fairly well defined from other points of view also (Ligurians, Bustocchians, Mandrognians and Sardinians). The data on the relative frequencies of the subgroups A_1/A_2 are rather fragmentary and are not always reported in a way that would permit gene frequencies to be derived from them. We can indicate, however, that the phenotypic ratio of the subgroups A_1/A_2 ranges from a maximum of 7 in Trieste to a minimum of 1 in the district of Sassari. Values of 6·4–5·4 have been observed for Milan and Ferrara, 4·5 for Venice and Vercelli, 3·2 for Ancona and 3·4 for Rome. In northern Italy, where low values are occasionally found, the exceptionally low value of 1·5

has been observed for Busto Arsizio and Trento. In general, the ratio decreases from the north to the south. Our only data from Sicily relate to the district of Catania (2·0–3·3). The ratio for the

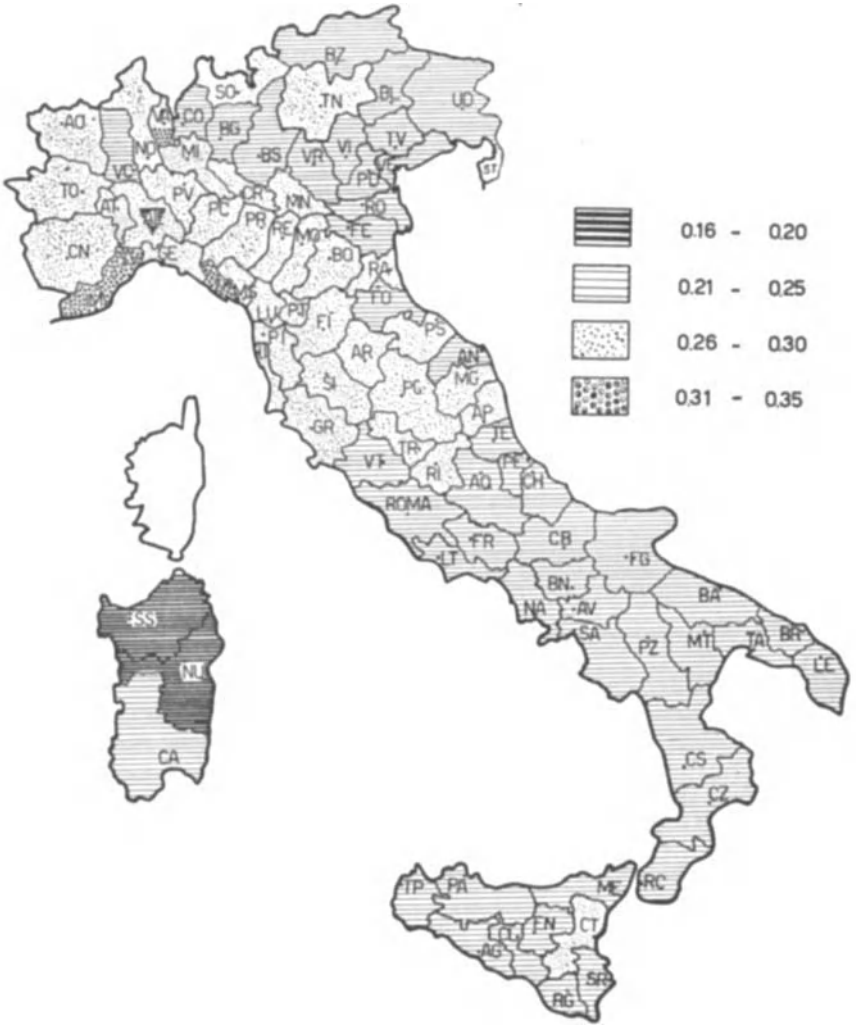

FIG. 1. Frequency of the gene $A(p)$.

district of Cagliari is 1·7, quite close to that observed in the Sassarese.

The highest frequencies of the gene $O(r)$ (Fig. 2) are found in

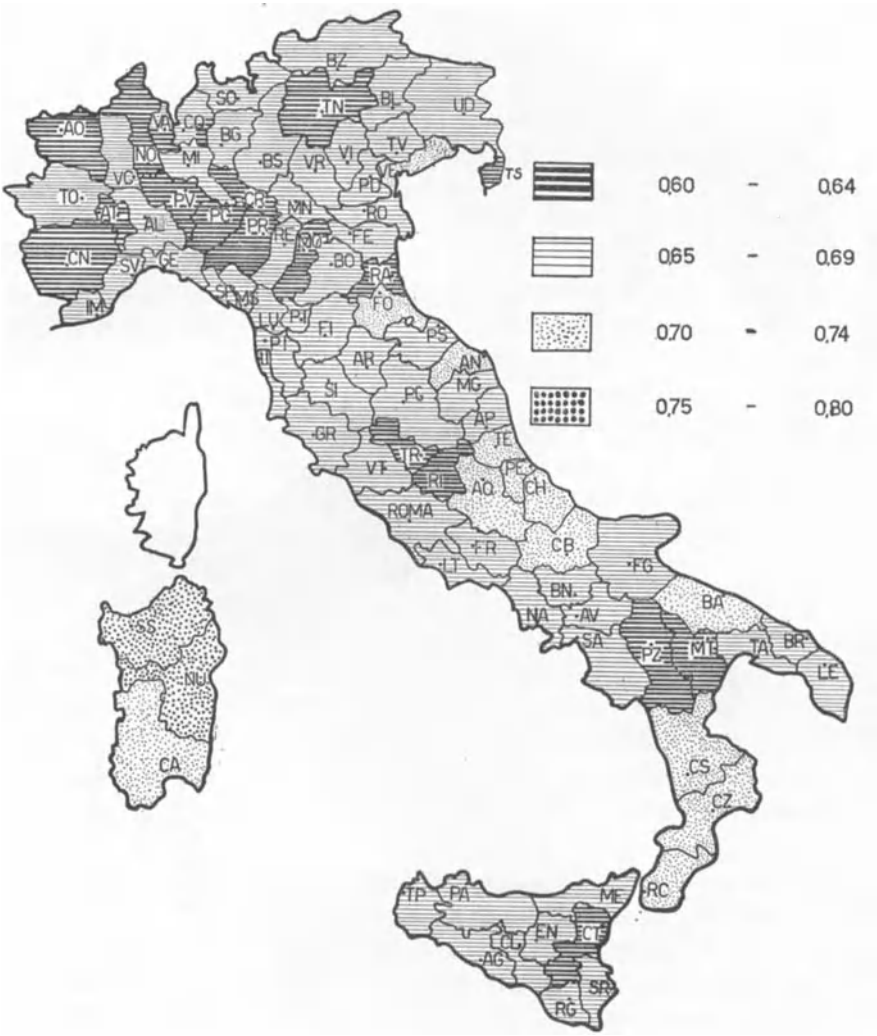

FIG. 2. Frequency of the gene $O(r)$.

northern Sardinia where they reach a level of 0·75–0·80. Moderately high values (0·70–0·74) have been observed in the district of Cagliari, in Calabria, in a number of districts along

FIG. 3. Frequency of the gene $B(q)$.

the Adriatic coast, particularly in Abruzzo and Molise (some reservations must be made for the data available for Venice) and in the Mandrogni. The lowest values (0·60–0·64) have been observed in the district of Catania, in Basilicata, in southern Umbria and in a number of districts in Emilia, Lombardy and Piedmont.

Fig. 3 shows the frequencies of the $B(q)$ gene. The lowest values (less than 0·05) have been reported for Liguria, the district of Ancona, and southern Umbria (with some reservations for the data concerning southern Umbria). Throughout the greater part of the country the values are between 0·05 and 0·09. Values of 0·10–0·14 have been observed for much of southern Italy (with the exception of Sicily), the southern Marche, and Trieste. (It must be pointed out that the frequencies indicated for the other eastern districts of Veneto are not derived from detailed studies and are only an expression of the "regional" frequency for the whole of Veneto.) The highest frequencies of the B gene (more than 0·15) are found in the Basilicata, and are probably an expression of the important Greek and Albanian settlements there.

Variations in the ratio of the frequencies of genes A/O are shown in Fig. 4. Because of the interference represented by the variations in gene B frequency, this ratio corresponds only partially to the absolute variations in frequencies of the A and O genes. The highest values are found in the Bustocchi, in Liguria and in parts of Emilia, and the lowest in northern Sardinia. Throughout the rest of the country the ratio tends to decrease from the north to the south.

For the MN system, the frequencies of the $M(m)$ gene are shown in Fig. 5. Data for the greater part of the country are lacking; of the frequencies observed, the lowest (0·50–0·54) have been reported for Trieste, Turin, Ancona, Naples and Catania. Slightly higher values (0·55–0·59) have been reported for Milan, Pavia, Genoa, Emilia, Pisa and Sicily; and values of

0·60–0·64 for the Bustocchi, Trento, Venice, Ferrara, Rome and Bari. The highest are for Sardinia: 0·70–0·74 in the district of Cagliari, 0·75–0·79 in the district of Sassari, and rising to as

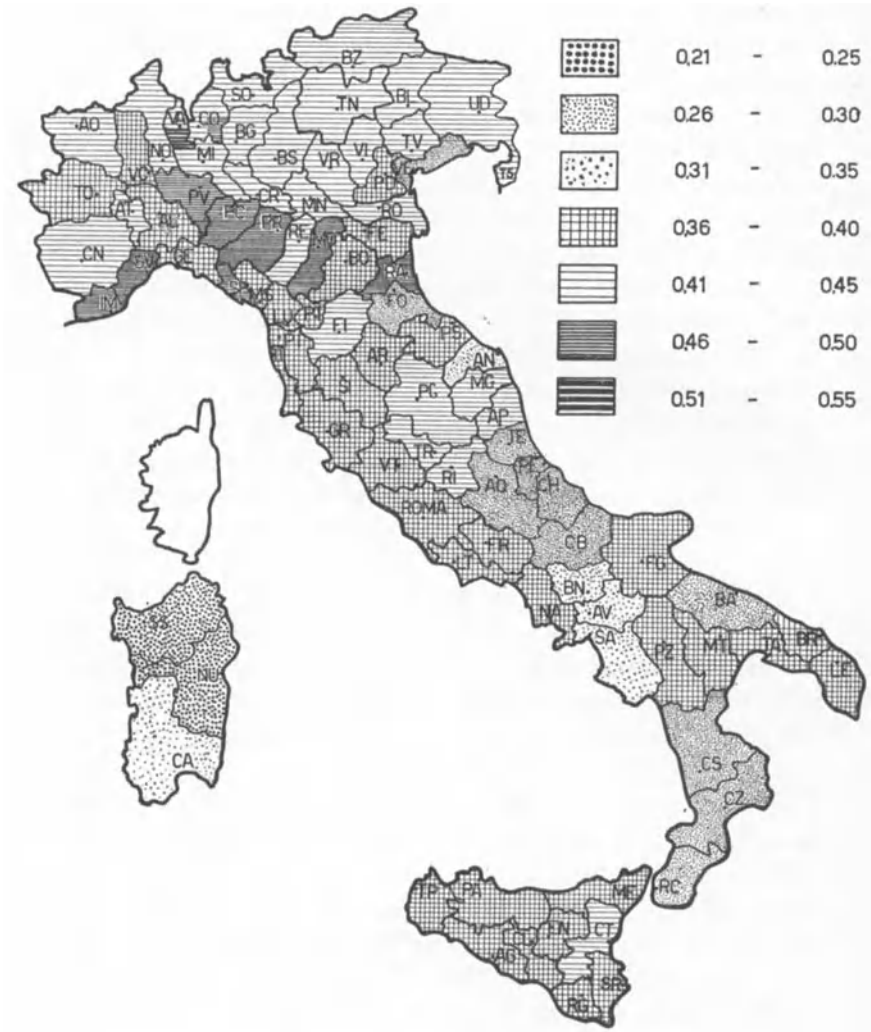

FIG. 4. Ratio of gene $A(p)$/gene $O(r)$.

high as 0·80 for Nuoro. It is noteworthy that such high gene *M* frequencies have been observed only for the Yemenites, Indochinese and Amerinds, from whom, however, the Sardinians

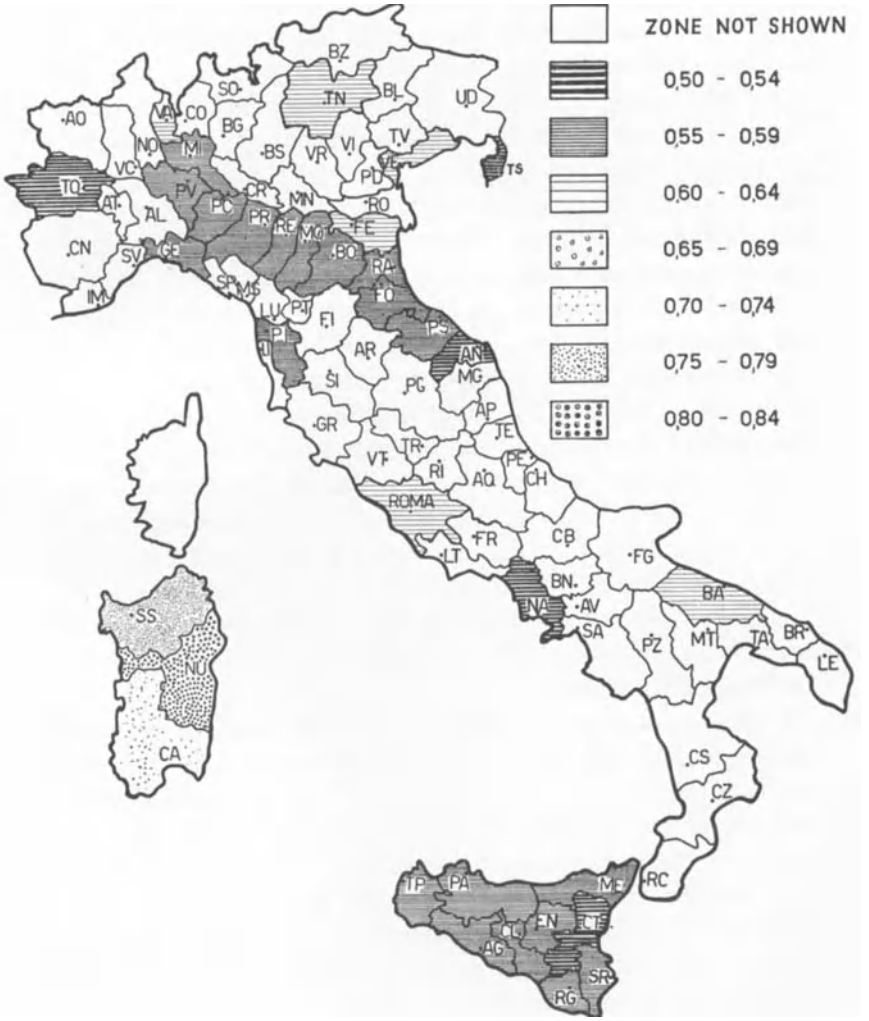

FIG. 5. Frequency of the gene *M*(*m*).

are clearly differentiated—also in this aspect—by the high frequency of the chromosomal combination *MS* which has no counterpart in any other population studied.

For the *Rh* system, Fig. 6 shows the frequencies of the $D(Rh_o)$ gene, which are the most informative and for which the most data are available due to the practical importance of this gene in the field of blood transfusion and obstetrics. The lowest values (0·51–0·55) are found in Liguria and seemingly in the district of Trento; values of 0·56–0·60 have been observed for the district of Veneto and in the neighbouring districts of Lombardy, and also in the districts of Ancona and Macerata. Throughout the remainder of northern Italy, the *D* frequency is between 0·61 and 0·65, except in the Mandrogni, at Erba, in the district of Como and in the Ferrarese where it is between 0·66 and 0·70. The frequencies found in Tuscany and in the district of Pesaro are similar to those in Piedmont and Emilia (0·61–0·65), but in the south of the peninsula and in Sicily higher values are found (0·66–0·70), with the exception, apparently, of Rome, Foggia and the three furthest districts of the Puglie (Taranto, Brindisi and Lecce) which have slightly lower frequencies (0·61–0·65), while those for the Basilicata and the district of Catanzaro are still higher (0·71–0·75). Sardinia has exceptionally high frequencies: again, values of 0·66–0·70 for Cagliari, and greater than 0·75 for the Nuorese and Sassarese.

Other blood group systems for which we have only scattered and insufficient data, and which therefore cannot be dealt with here, are the Lutheran, Kell, Lewis and Duffy systems. Although more data are available concerning the *Pp* system, these are subject to some serological reservations. However, the values observed for the *P* gene range from 0·41 (Rome) to 0·64 (Milan), with values of 0·41–0·45 for Venice and Ancona, 0·46–0·50 for Ferrara and Turin, 0·51–0·55 for Trieste, Pisa and the Puglie, and 0·56–0·60 for Florence and Catania.

It is clear that as far as central Italy is concerned—which is the

most interesting region from the point of view of this symposium —we have not enough reliable data to allow us to draw either positive or negative conclusions about the existence of differences

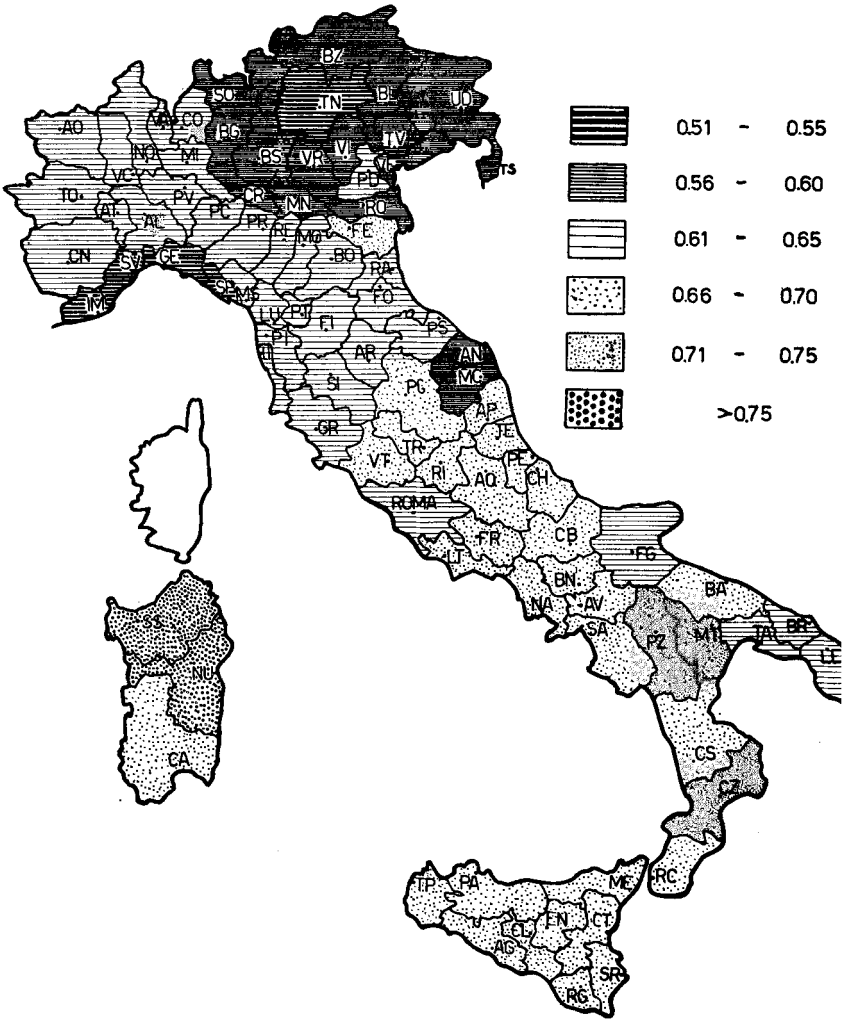

FIG. 6. Frequency of the gene $D(Rh_o)$.

in blood group distribution, which could eventually be related to the ethnic influence of the Etruscans.

Therefore, I suggest that the members of this symposium formulate and send to the National Committee for the Study of Blood Groups in Italy, a proposal for a systematic study of the populations of Etruria and the adjacent zone, the latter to furnish eventually material for comparative purposes.* Likewise, a study of ancient populations could be made in parallel with the research on living populations, when a successful method is developed for blood group determination in skeletal material.

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MOURANT, A. E. (1954). The Distribution of the Human Blood Groups. Oxford: Blackwell.

DISCUSSION

Mourant: Prof. Morganti has raised a number of very important points, and has indicated some of their wider implications. I would support very strongly his suggestion that we should express some collective view. The results he has obtained so far are so interesting and at the same time so tantalizing that if we can do anything by our collective expression of opinion to encourage the Italian authorities to help him I should be very glad to support such action.

Bolelli: Although I had my doubts and misgivings, and was one of the most sceptical amongst those present, I was most impressed by the distribution of gene *A*. This corresponds roughly, in some points, to the distribution of dialects, i.e. we saw some differences which we attributed to the substratum of the languages as in the case of aspiration in Tuscany. One very important point in the study of the substratum is the problem of the Ligurians. In almost exactly this territory, and

* A resolution was passed by the members of the symposium and the text of it is given on p. 204—Eds.

also in some places in southern Provence, an important phonetical fact is the palatalization of *r*. This phenomenon is found only in this particular territory extending from La Spezia to the French border and beyond that into some parts of Provence. Historical data say that the Ligurians had spread very far, but in this area the Ligurians must have persisted for a longer time. The linguistic situation is fairly similar to that shown by Dr. Morganti's map, for instance, in Sardinia where there is considerable difference between the North and the South Sardinian dialects; and here again we have a different distribution.

Another important point emerges if we make linguistic maps, and consider, for instance, the Celtic ethnic substratum, which phenomenon is most obvious in French; the Latin *d* becomes *é*. Another surprising point is that we find an absolutely compact area (central southern Italy) including Rome and southern Italy where *mb* is pronounced *mm* and *nb* becomes *nn*. There are certain points where that does not correspond. Now, at least, I find a certain positive element of correspondence with blood group distribution, although I still believe that the linguistic phenomena are absolutely independent of racial, genetical or anthropological factors, because language is spread in such a manner that if one takes completely different types of people at a certain stage, and lets them learn exactly the same language, they will speak it in the same way. It is enough to consider how English has developed. For instance, in the development of Latin one can see that the language is quite independent of biological and racial factors.

Morganti: It is most interesting that you extend this seemingly to South Provence. I think this corresponds to the gene *A* distribution, but it would not be possible to see gene *A* on a map of this scale; but it does indicate the particular areas which should be studied more in detail, e.g. along the boundaries of southern Italy.

Mourant: The question of the Ligurians has been raised, and I was impressed by the fact that, from the *Rh* map, the highest frequency of *Rh* negative in Italy appears to be in Liguria; the other part of Europe where a similar high frequency of *Rh* negative is found is the Basque country. Is there any possible connexion?

Oakley: Is the Ligurian language non-Indo-European?

Bolelli: The Ligurians are a problem. We have very few data available; we can see one group with a pre-Indo-European language, and

one group of Indo-Europeanized Ligurians. From the available data we can distinguish two distinct languages, a pre-Indo-European Ligurian and an Indo-Europeanized Ligurian.

Ceppellini: As regards the relationship between biology and linguistics, it is important to be aware that we are, in fact, dealing with two different kinds of inheritance, biological and cultural. Obviously, the two sets of observations are independent of one another in the sense that a particular language, for instance, does not necessarily imply particular biological features (e.g. features of vocal cords) but on the other hand, these two are interrelated, perhaps only in a spurious way, and their interrelationships should be carefully studied.

Bloch: Interrelationships between biological and linguistic factors may refer only to phonetics and are always very dubious. Some anatomical feature might influence the mode of speaking in very rare cases, but certainly not the structure of the language. Prof. Bolelli will probably agree with me.

Bolelli: When we speak of the substratum, we always refer to phonetics.

Ceppellini: We do not necessarily mean that there is a physiological relationship, but we know that there is a relationship of some kind.

Banti: From the archaeological point of view, in reference to northern Italy which is the region that I know best, I have found one possible correspondence between biological and archaeological data, much like that discussed by Prof. Bolelli from the linguistic point of view. Archaeologically, the Ligurian coast has its own distinctive character (excepting Genoa where one finds foreign influences—Greek, Etruscan etc.); and this led to a number of archaeological and ethnic theories. Moreover, the point on Dr. Morganti's map which corresponds more or less with the river Ticino in the Province of Varese, is where we find a series of tombs, especially in eastern Liguria. It is difficult to see how this relationship came about. The tombs in the Swiss Canton of Ticino have puzzled scholars; it has been suggested that they are of Ligurian origin.

Barnicot: As regards the general question of the connexion of languages with biological characters, the simplest way to look at it is that they very often have a common factor in the degree of isolation of population, either geographical or social. For instance, take the case

of the Australian aborigines: they may have been a population isolated for long periods on a continent; it is not surprising that in such a population there is, on the one hand, a fair degree of biological differentiation proceeding by its own mechanism and, at the same time, a considerable amount of linguistic differentiation. By the same token, if you have a population with different physique and a different language entering a new area, then, provided the population remains isolated from biological or cultural admixture with its neighbours, there remains a correlation between physique and language. As Prof. Ceppellini says, the correlation is interesting from the point of view of historical interpretation. But from the point of view of causation, in the direct sense, it is a spurious relationship.

Cook: In other words, accidental but significant.

Ward Perkins: I agree whole-heartedly with Dr. Barnicot's remarks; but it does seem to me that there has been a certain equivocation in some of the discussion today. At the present moment we should not be looking for causal factors. Our medical friends have been extremely candid in stating that genetical research is in a very preliminary phase. Probably my fellow-archaeologists would be equally candid in stating that our archaeological knowledge is really extraordinarily fragmentary in a number of essential points. The same may even be true of the philologists. Two factors which are most likely to link, at the present juncture, are the archaeological and genetical data. All our thinking, at present, ought to be in terms of simple factual juxtaposition, not in terms of any causal relations, and not in terms of historical interpretation. On that basis we may or may not be able to produce some significant results. But if we introduce these other complicating elements, we are only laying up further trouble for ourselves.

Cook: Most of us agree that it is better to collect the evidence before deciding what it means!

Mourant: On the other hand, we have not the money or the man power to do everything; so we must be guided both in archaeology and in blood group studies to some extent by what is likely to have relevance in the future.

Bolelli: Although we recognize that much work remains to be done it is true that, as far as Italian philology is concerned, a great deal has been done. Many linguistic dictionaries have already been published.

One has been published, for instance, by the Swiss workers, and another major work is now in course of preparation by the Italians, which will cover very thoroughly the whole field of Italian dialects. We already have about 1000 classified dictionaries of dialects, and these studies have been carried out over more than one hundred years.

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RESOLUTION

The members of the symposium, research workers of various nationalities and different disciplines, aware of the value of studies on the frequency of distribution of blood groups for the understanding of important—and so far insufficiently investigated—points in common between the different ethnic groups, address a resolution to the National Committee for the Study of Blood Groups in Italy, formed under the presidency of the High Commissioner for Hygiene and Public Health, proposing that special research should be undertaken on the populations of Etruria and the neighbouring zones which are recognized as being of great interest both for the biological disciplines and for those of history and archaeology.

Medical Biology and Etruscan Origins
G. E. W. Wolstenholme,Cecilia M. O'Conner
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EFFECT OF A SINGLE GENE DIFFERENCE ON THE PATTERN OF SOME PHYSICAL MEASUREMENTS*

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MORANT (1939) said that "the main aim of physical anthropology is to unravel the course of human evolution and it may be taken for granted today that the proper study of the natural descent of man is concerned essentially with the mode and path of his descent".

This statement is fully accepted by the majority of anthropologists today but they still are faced with the difficulty of deciding which physical characteristics to choose in classifying man and, in particular, which can be considered to be of importance with regard to the problem of tracing the racial history of man.

It is obvious that such characteristics should not be too ephemeral, that is they should not be, for instance, too susceptible to possible modifications by environmental and cultural influences. On the other hand it would be desirable, if we are interested in early human history, to deal with characteristics which can be identified in ancient human remains. In order to meet the first requirement it is clear that we should always deal with inherited characteristics, but it is, on the other hand, known that not all of them are equally suitable in this respect.

It is generally agreed that the distribution of inherited characteristics may be more or less influenced by selection, that is to say

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that some characters may be better adapted than others to a particular environment and, therefore, the gene or genes which are responsible for them tend to increase in their relative frequencies.

It is clear that the differential adaptiveness of inherited characteristics to different environments limits in some way their efficiency as means to trace back the racial history of man in the sense that it is conceivable, for instance, that the distribution of gene frequencies for any genetical character is likely to be altered if the ecological background of a human group changes or if it migrates from a given environment to a different one.

On the other hand, less adaptive or non-adaptive characters—if they exist—would not be much more helpful in this respect because, without adaptation, there would perhaps be no differences at all in the distribution of genetical characters among different human groups other than those due to random drift which, of course, are rather meaningless as far as the problem of tracing human history is concerned.

Among physical characteristics, the importance of studying the patterns of some body measurements has been particularly emphasized in the past. One of the strongest arguments in favour of this approach in anthropology is the possibility of extending the investigations to skeletal material.

Some authorities have also expressed the feeling that bones, because they are "hard", are less influenced by environmental forces. This is clearly too simple a view but, as Morant stated, the "anthropologist must hope that this is so, for if skeletal characters are unstable, there can be little hope of finding solutions to the problems of human ancestry which can only be approached directly by using skeletal material".

Unfortunately, there is enough evidence today (Schreider, 1950) to make it more and more probable that many metrical characters are highly adaptive to climate. This is also true for skin and hair colour.

It is, furthermore, hardly necessary to emphasize the possible influence that some environmental agents, such as sanitation, infectious diseases, food, may play as factors controlling the growth of skeletal parts of the body and as selective forces.

Other common disadvantages are the difficulty of determining accurately some of the morphological characters of the skeleton in living human beings and the gross lack of information concerning their exact manner of inheritance.

In the last fifty years genetical research has provided the modern anthropologist with a new set of inherited characteristics. These are mainly represented by blood group antigens, haemoglobin variants, thalassaemia and, recently, by plasma proteins.

The chief advantage of these new tools for physical anthropology lies in the fact that their genetical determination is very simple and fairly well understood.

There is no doubt that this is of great importance when studying the trend of their distribution in natural populations, but it could be long disputed whether they are to be considered less or more adaptive than the metrical characteristics of classical anthropology.

Furthermore, as Haldane remarks (1956), there is no character, among these new tools that genetics has handed over to anthropology, which sharply differentiates two populations, as skin colour or hair structure may, although it is true that some antigens or combinations of antigens are rare and probably absent in some populations and common in others.

It is, therefore, clear that neither classical anthropometry nor population genetics of simple Mendelian characters could solve by themselves the problem of unravelling the course of human evolution.

Nevertheless, there is no doubt that human biology may contribute very useful information to ethnology on a short-term basis. On this scale, even strongly adaptive characters might be of some use, provided that enough is known about their genetical determination.

It is to be hoped that, in the next few years, the amount of data concerning the distribution of gene frequencies for simple Mendelian characters, on the one hand, and classical anthropometric observations, on the other, will considerably increase.

As Mourant (1954) emphasizes, in his valuable book on the distribution of blood groups, it is only in this way that a better understanding of the problem might be achieved, especially considering the possibility that the comparison of the two classes of data could "enable a better estimate to be made than formerly of the amount of genetical information comprised in the classical material".

It was with this idea in mind that we investigated the distribution of a certain number of physical measurements in a random sample of thalassaemic families collected from the district of Ferrara (Italy). As is well known, thalassaemia* is due to a single autosomal gene pair. Heterozygous individuals show a mild form of hypochronic anaemia usually compatible with good health. The homozygous are affected, almost at once after birth, by a very severe anaemia, thalassaemia major or Cooley's disease, which leads to death before the age of reproduction.

The gene for thalassaemia appears to be most frequent around the Mediterranean, though it has recently been found in several other parts of the world, including Indochina and Indonesia (Brumpt, De Traverse and Coquelet, 1956).

The highest frequencies have been reported in Greece, Cyprus and Italy. The distribution in Italy is quite uneven. There are some areas, like the districts of Ferrara, Sardinia, Calabria and Sicily, where the frequency of heterozygotes reaches levels of between 10 and 20 per cent.

* There is a great deal of controversy on the terminology of thalassaemia. The present authors have used, on other occasions, the terms Microcythemia for thalassaemia minor and Cooley's disease for thalassaemia major. This is the terminology put forward by Silvestroni and Bianco (1943, 1949). In the present paper the most current, though not necessarily the most pertinent, terminology in use in world medical and genetical literature has been adopted. For further details see Montalenti (1954).

The cause of this heterogeneity in the distribution of thalassaemia is still obscure.

Many speculations have been made as to the possible common racial origin of thalassaemic populations (Gatto, 1942; Silvestroni, Bianco and Alfieri, 1952). But since we are dealing with a gene lethal in homozygous conditions, it is obvious that the "migration theory" by itself is not enough to explain the high levels of gene frequencies which are found today in some areas.

It is necessary, therefore, to postulate a counterbalancing mechanism capable of compensating the continuous loss of the gene through the early death of the Cooley homozygotes. Montalenti and co-workers (see Montalenti, 1954) believe that this could merely consist in a higher adaptive value of the heterozygotes as compared with the normal individuals. Haldane suggested in 1949 that they could be more resistant to malarial infection, known to have been highly endemic in the areas where now a high frequency of thalassaemia is observed. Ceppellini (1955) has recently collected some data in Sardinia which support this hypothesis. If this is true, it is to be expected that the frequency of thalassaemia will drop in the course of a few generations in those areas where malaria has been completely eradicated. A trend of this sort seems already to be noticeable in the district of Ferrara (Montalenti *et al.*, 1957).

With regard to the hypothesis of a common racial origin, as far as the available data on the world distribution of thalassaemia go, it seems more and more evident that this mutation is likely to be present in all racial groups (perhaps in different allelic forms) and that the differences in gene frequencies might be explained in terms of adaptation. The "migration theory" is therefore not an indispensable one but, although in some cases (Ceppellini, 1955) it seems possible to rule it out on the basis of the information available on the distribution of other genetical characters, there is no reason to exclude that it might be of some importance in others.

Silvestroni, Bianco and Alfieri (1952) emphasize the fact that the region of higher frequency in southern Italy corresponds fairly well to Magna Grecia and that the region of the Po delta, where the gene reaches the highest peak, is known to have been a flourishing centre of Greek colonization in the centuries B.C. According to these authors the population of this region, which received a constant flow of Greeks from the sea during early times, and through contact with the Byzantine empire in a later epoch, was prevented from intermingling with the waves of northern populations coming southwards during the Middle Ages by the particular topography of the Po delta.

We felt therefore that, together with the data on the distribution of blood groups and other simple genetical characters, an investigation based on the classical tools of anthropometry might have some value in this population in relation to the problem of tracing their past history, especially if the data collected were going to be compared with similar ones to be obtained from neighbouring non-thalassaemic areas.

THE DATA

A sample of 388 families with a total of 1,951 individuals was investigated for the following anthropometric measurements: weight, stature, interpupillary and interbizigomatic distance, head breadth, head length, facial height and facial breadth.

These families were all collected from a village in the district of Ferrara (Berra) which had previously been examined with complete ascertainment and where the frequency of thalassaemia was found to be of the order of 15 per cent (Silvestroni and Bianco, 1957; Montalenti *et al.*, 1957).

Among these families, 241 represent all those found to have one parent thalassaemic, 24 those with both, while a random sample of 123 families has been taken from the total of 704 families with both parents normal.

Table I shows a summary of means and S.E. for all the set of

Table I

DISTRIBUTION OF SOME BODY MEASUREMENTS IN A SAMPLE OF NORMAL AND THALASSAEMIC INDIVIDUALS OVER 20 YEARS OF AGE

	Weight	*Stature*	*Inter-pupillary distance*	*Inter-bizigo-matic distance*	*Head breadth*	*Head length*	*Facial breadth*	*Facial height*
Males								
N (336)	68·9	167·2	63·2	131·4	157·6	186·2	131·3	119·6
	±0·57	±0·36	±0·15	±0·34	±0·36	±0·34	±0·41	±0·46
Th (183)	67·8	168·3	63·5	132·3	158·3	187·2	132·2	119·8
	±0·73	±0·52	±0·21	±0·47	±0·41	±0·47	±0·47	±0·54
Females								
N (327)	62·9	154·6	60·9	122·9	151·1	177·7	122·3	110·1
	±0·60	±0·32	±0·17	±0·33	±0·29	±0·33	±0·43	±0·35
Th (217)	61·4	154·5	60·9	123·3	151·2	178·1	123·2	109·9
	±0·64	±0·40	±0·23	±0·52	±0·43	±0·41	±0·61	±0·47

N = Normals. Th = Thalassaemics.

measurements performed among adult individuals (over 20 years of age), grouped for sex and thalassaemia. It appears that thalassaemic individuals tend to be lighter, taller and to have a slight increase in all cephalic diameters. We shall analyse, in the present instance, only the distribution of the first two characters, weight and stature. A full account of the analysis of the other available data will be presented later elsewhere.

As may be noticed at first glance at Table II, the differences in the means for these two characters are not very impressive, but it must be remembered that weight and stature are positively correlated and therefore a change in the pattern such as the one found is, in fact, more significant than it might appear from just comparing the means of the two characters in the groups of normal and thalassaemic individuals.

For a better estimation we have, therefore, combined these two sets of measurements in a discriminant function (L), calculated according to the classical method suggested by Fisher (1936) for characters which are approximately normally distributed and share the same variances and covariances. Since we are

dealing with metrical characteristics obviously different in the two sexes, the discriminant function (L) had to be worked out separately for males and females. As shown in Table II, we then found:

among males: $L_{\delta\delta} = 0{\cdot}589$ weight $- 1{\cdot}0$ stature,
among females: $L_{\varphi\varphi} = 1{\cdot}0$ weight $- 0{\cdot}141$ stature.

Table II

DISTRIBUTION OF WEIGHT AND STATURE IN A THALASSAEMIC POPULATION* (DISTRICT OF FERRARA, ITALY)

	Males		*Females*	
	Normals	*Thalassaemics*	*Normals*	*Thalassaemics*
Number	336	183	327	217
Weight (w) $\pm$ S.E.	$68{\cdot}9 \pm 0{\cdot}57$	$67{\cdot}8 \pm 0{\cdot}73$	$62{\cdot}9 \pm 0{\cdot}60$	$61{\cdot}4 \pm 0{\cdot}64$
Variance (w)	108·02	97·23	116·98	89·24
Stature (st) $\pm$ S.E.	$167{\cdot}2 \pm 0{\cdot}36$	$168{\cdot}3 \pm 0{\cdot}52$	$154{\cdot}6 \pm 0{\cdot}32$	$154{\cdot}5 \pm 0{\cdot}40$
Variance (st)	44·61	48·72	33·56	34·46
Covariances	30·21	39·45	17·05	8·23
Discriminant function (L)	$+0{\cdot}589w - 1st$		$+1w - 0{\cdot}141st$	
$\bar{L} \pm$ S.E.	$-126{\cdot}6 \pm 0{\cdot}37$	$-128{\cdot}4 \pm 0{\cdot}44$	$+41{\cdot}1 \pm 0{\cdot}59$	$+39{\cdot}6 \pm 0{\cdot}64$
$(\bar{L}_N - \bar{L}_{Th}) \pm$ S.E.	$+1{\cdot}8 \pm 0{\cdot}58$		$+1{\cdot}5 \pm 0{\cdot}87$	
t	3·13		1·75	
P	$<0{\cdot}001$		$<0{\cdot}10$	
			$>0{\cdot}05$	

* All individuals tested were over 20 years of age.

Note: Owing to the reasonably large numbers involved it is sufficiently correct to consider the difference $L_N - L_{Th}$ divided by its S.E. as "t" with an infinite number of degrees of freedom.

Using these equations, it follows that the mean L-values in the group of thalassaemics are significantly lower than those observed among normals, and that the level of significance is somewhat higher for males.

This situation might be perhaps better illustrated by the graph shown in Fig. 1 where the effect of thalassaemia on the pattern of these two physical measurements is shown for the two sexes and

for different age groups. The line "L" is a typical separation line, "isocritic line", for discriminating between thalassaemics and normals: i.e. if we wish to draw a line separating as well as possible the two groups of individuals (in the whole population, regardless

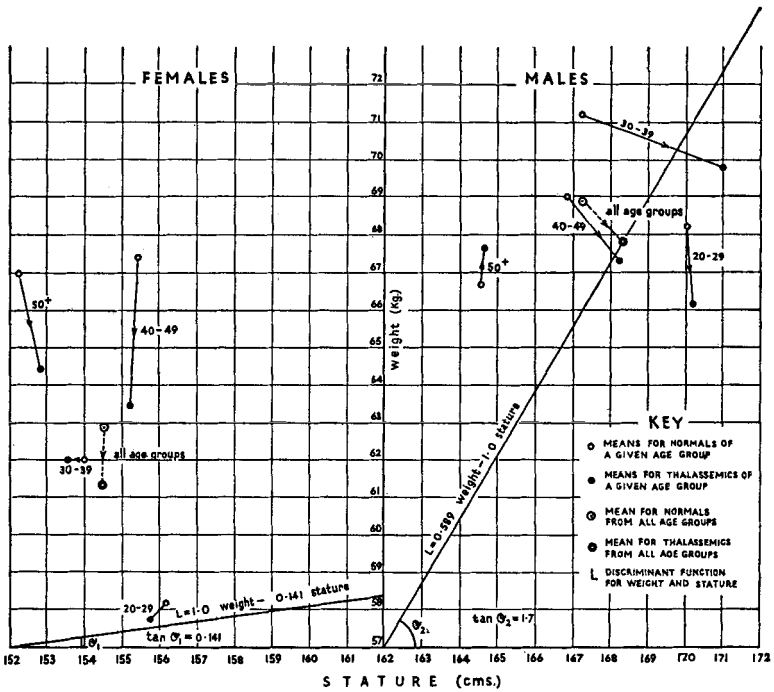

FIG. 1. Distribution of means for weight and stature in a sample of 663 normals and 400 thalassaemics.

of age and sex) we should take a parallel to the given line (Smith, 1947).

Here, also, it may be clearly seen that this effect is more noticeable among males and for the age groups between 30 and 49 years. Among females the effect is self-evident only for the group between 40 and 49 years and it practically refers to weight alone.

But in all cases (except for one age group in each sex) the line joining the means goes in roughly the same direction.

It is conceivable that these differences with age and sex might be due to the interference of some other physiological factor (as for instance, the maternity period for women and senescence for both sexes) which, by grossly altering the pattern of almost all body measurements, may minimize the effect of thalassaemia on weight and stature.

On the other hand, it is also conceivable that the population investigated might not be an entirely homogeneous mixture, so that in one part there might be for some reason a tendency to have a high incidence of thalassaemia, greater stature and lower weight, and in another part these conditions could be reversed. Such a state of affairs would give an association between thalassaemia and tallness and low weight which would be purely statistical in origin and not physiological or genetical.

In order to obtain a critical proof of the real existence of the effect it seemed, therefore, better to see if it existed within family groupings, i.e. where thalassaemic individuals were compared with their normal siblings.

The quantity compared was: (weight in kg. — stature in cm.). This is very similar to the discriminant function previously calculated and is simple to compute.

In order to allow for the different ages and sexes of the siblings, this was standardized for each individual by the calculation of:

$$\Theta = \frac{\binom{\text{individual}}{\text{weight}} - \binom{\text{individual}}{\text{stature}} - \text{mean (weight} - \text{stature)}^{\star}}{\text{standard deviation (weight} - \text{stature)}^{\star}}$$

Thus, we have to see if there is a tendency for the mean value of Θ to be lower among thalassaemics than among normals. Of course, in principle, the calculation could be made more strongly

* For corresponding sex and age group.

significant by using, instead of Θ, the discriminant function for the particular age and sex group of the individual concerned, since this is, by definition, the function which distinguishes most sharply between normals and thalassaemics, in the data observed.

However, this could bias the calculation in favour of getting a significant result, and it is difficult to correct for this bias, since the situation is a complicated one in which different individuals in the same sibship will have different discriminant functions. Thus, it seems simpler to keep to the single function "weight – stature" (suitably standardized); although this has been chosen to be similar to the discriminant, this will only introduce a small bias in favour of significance. Fisher (1936) corrects for the bias in a similar case by a small adjustment to the degrees of freedom and, in a large sample, this adjustment can be neglected.

It follows that (neglecting such a bias) we can consider that, in each family, either the effect we are looking for is present or there is no association between the values of Θ and the presence of thalassaemia. Thus, for example, if a family contained three children of whom one was thalassaemic and the Θ-values were Θ_1, Θ_2, and Θ_3, the "null hypothesis" is that it is equally likely that the Θ-value of the thalassaemic is Θ_1, Θ_2 or Θ_3. On this basis one can calculate that, if

$\bar{x}$ = (observed) mean of Θ-values of thalassaemics in the family, and

m = (observed) mean of Θ-values of all individuals in the family,

then $E(\bar{x})$, the expected mean of $\bar{x}$, is m;

e.g. if the hypothetical family of three, with Θ-values Θ_1, Θ_2 and Θ_3, occurred many times with the thalassaemic individual being equally often Θ_1, Θ_2, or Θ_3, then $\bar{x}$ would be equally often Θ_1, Θ_2 or Θ_3, and so, on the average, $\bar{x}$ would have the mean:

$$[\Theta_1 + \Theta_2 + \Theta_3]/3 = \overline{m}.$$

$$\text{Also the variance of } \bar{x} = V\left(\frac{1}{r} - \frac{1}{n}\right)$$

where r = number of thalassaemics,

n = total number of individuals,

$$V = \text{variance of } \Theta \text{ in family} = \left[\Sigma\Theta^2 - \frac{(\Sigma\Theta)^2}{n}\right] / (n-1).$$

(That is, the mean and variance are calculated on the basis of the observed Θ-values being regarded as fixed, and the thalassaemia distributed at random in the family. This has the advantage of giving an exact "non-parametric" test, i.e. one which does not depend on the normality or otherwise of the distribution of Θ.)

Unfortunately, only 36 families (with a total of 112 children, 57 of whom were thalassaemics) were available for this family study since all non-segregating families and the ones with children under 20 years of age had to be excluded.

In combining the information from all the families, it is advisable to give greater weight to larger families since they are more informative. The best weight to give seems to be r, the number of thalassaemics in the family, so we consider $A = \Sigma\, \bar{x}r$ summed over all families. This has expected value $B = \Sigma \bar{m}r$ and variance $C = \sum V\left(\frac{1}{r} - \frac{1}{n}\right)r^2$, so that

$$\frac{A-B}{\sqrt{C}}$$

will be approximately a normal variable on the null hypothesis. If the effect is present, however, this should be significant according to the tables of the normal distribution.

The result has been that (B), i.e. the sum of scores of thalassaemic individuals for the weight–stature measurement, appears to be significantly smaller at a level of 0·05 than the corresponding

sum of scores for normals, (A), when compared in terms of their common variance (C):

$$\frac{(A)-(B)}{\sqrt{(C)}} = \frac{(-4\cdot810)-(-13\cdot050)}{\sqrt{17\cdot712}} = 1\cdot96.$$

The data from which these calculations have been derived will be published later in full in the *Annals of Human Genetics*, together with a more complete analysis of all other measurements performed.

Table III

DISTRIBUTION OF THE DISCRIMINANT FUNCTION (L) IN DIFFERENT AGE GROUPS

($L_{\delta\delta} = 0\cdot589$ weight $- 1\cdot0$ stature; $L_{\varphi\varphi} = 1\cdot0$ weight $- 0\cdot141$ stature)

	Age groups (years)							
	20–29		30–39		40–49		50+	
	Normals	*Thalassaemics*	*Normals*	*Thalassaemics*	*Normals*	*Thalassaemics*	*Normals*	*Thalassaemics*
$\bar{L}_{\delta\delta}$	$-129\cdot9$	$-131\cdot3$	$-125\cdot4$	$-129\cdot9$	$-126\cdot2$	$-128\cdot4$	$-125\cdot3$	$-124\cdot8$
$\bar{L}_N - \bar{L}_{Th}$	$+1\cdot4 \pm 1\cdot04$		$+4\cdot5 \pm 1\cdot18$		$+2\cdot2 \pm 0\cdot99$		$-0\cdot5 \pm 1\cdot14$	
P	$> 0\cdot20$		$< 0\cdot001$		$\sim 0\cdot02$		———	
$\bar{L}_{\varphi\varphi}$	$+36\cdot1$	$+35\cdot7$	$+40\cdot2$	$+40\cdot4$	$+45\cdot4$	$+41\cdot5$	$+45\cdot5$	$+42\cdot9$
$\bar{L}_N - \bar{L}_{Th}$	$+0\cdot4 \pm 1\cdot07$		$-0\cdot2 \pm 1\cdot52$		$+3\cdot9 \pm 2\cdot15$		$+2\cdot6 \pm 2\cdot20$	
P	———		———		$\sim 0\cdot05$		———	

SUMMARY AND CONCLUSIONS

It is obvious, from the data presented here, that thalassaemia has some effect on the pattern of some body measurements and mainly on weight and stature. Since the effect is a dilute one and, furthermore, rather limited by age and sex, it is just conceivable that it does not represent a direct pleiotropic effect of the thalassaemic gene but rather a more remote consequence of the possible difference in fitness of normal and thalassaemic individuals living in the investigated area.

Certainly the problem requires a more careful and extensive study but, nevertheless, it seems worth while, at this stage, to

emphasize some points of general interest in the field of genetical anthropology that such results imply.

First of all it is clear that, in the population concerned, there is little hope of getting any definite answer regarding its ethnic derivation by studying the distribution of the classical anthropometric characters, because they seem to be clearly affected by thalassaemia itself. It is, therefore, likely that the often reported finding that thalassaemic populations show somewhat peculiar facial characteristics is not to be interpreted as a sign of racial intermixture. Of course, all this does not throw much light on the ethnic derivation of the population concerned; this still remains a matter of opinion, but it seems to us that we might be justified in considering our results as a first example of how an investigation based on Mendelian genetics and classical anthropometry combined together may, in fact, enable us to get some more definite answer, no matter if a rather negative one as in the present case, in the field of genetical anthropology.

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DISTRIBUTION OF SERUM HAPTOGLOBIN TYPES IN SOME ITALIAN POPULATIONS

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UP till now the greatest contribution of human genetics to anthropology and ethnology has been in the study of the blood group differences. It has emerged that human beings are enormously diverse in the pattern of antigenic substances which they possess on the surface of their red blood cells and it is probably almost true to say that with the exception of identical twins no two people are exactly alike in this respect. Nevertheless, the individual components which go to make up this complex pattern are susceptible to genetical analysis in quite simple terms, and the frequencies of the various genes responsible may be estimated in different populations so that, as Mourant (1954) has shown, it is possible to construct maps of these gene distributions which provide much food for thought when considered in anthropological terms.

Now the antigenic substances on the red cell surface are complex macromolecules, and the differences in their immunological specificities imply differences in their chemical structure. This in turn implies differences, possibly very subtle ones, in the character of the biosynthetic processes concerned in their formation. It is possible, for example, that the different genes responsible for the *ABO* blood groups determine the formation of different kinds of enzymes which are somehow involved in the synthesis of the mucopolysaccharide molecules which Morgan and Watkins have shown to possess the characteristic immunological specificities of this blood group system.

It would be rather surprising if such diversity in biosynthetic processes were peculiar to the blood group substances. In fact, it seems possible that similar genetically determined differences may be a common phenomenon in connexion with the formation of many other sorts of macromolecules which the individual usually synthesizes.

If this is so, then it may be that the main trend of future work in this field will be in the study of such individualities in human biochemistry, and of their frequency distributions in various human populations and ethnic groups.

It is possible that the contrast which exists at the moment between the great amount which is known about blood group differences and the paucity of information about normal variation in the synthesis of other substances has, to a large extent, arisen because of technical reasons. The serological techniques available for the characterization of red cell antigens do not, in general, require very complicated apparatus and are capable of being applied to population survey work with reasonable facility and speed. The physicochemical and immunological techniques which would be involved in studying other kinds of macromolecular differences have, however, in the past been rather complicated, cumbersome and on the whole not suitable for the investigation of large numbers of individuals in families and populations, as is required in this kind of work. Nevertheless, the situation is changing and it seems possible that, with the extensive application of some of the recently developed physicochemical techniques of separation to the study of macromolecular variation in normal subjects, the position will become very different.

One example of this, which is perhaps the forerunner of many others, is the discovery of the inherited difference in the formation of certain plasma proteins, recently made by Smithies.

Smithies (1955) developed a new method for the electrophoresis of plasma proteins, which resulted in a much greater degree of resolution of the different protein fractions than it had been

possible to obtain hitherto by any of the electrophoretic methods which were available. It involved electrophoretic separation in a medium of gelled starch. Under these conditions the resolution obtained appears to depend not only on the "true" free solution mobilities but also to a considerable extent on the molecular dimensions of the different protein species present.

Smithies found that when plasmas from a series of normal individuals were examined with this technique, three qualitatively different types of protein pattern could be recognized. The differences between the three types of pattern were due to the behaviour of a particular group of proteins which have been called haptoglobins because they possess the specific property of forming stable complexes with haemoglobin. The three types are now referred to as haptoglobin types 1.1, 2.1 and 2.2 (Smithies and Walker, 1956), although originally they were called I, II A and II B, respectively. Only a single haptoglobin component can be observed in the plasma of type 1.1. In the plasmas of types 2.1 and 2.2, however, several haptoglobin components occur, those in the 2.1 plasmas having somewhat different mobilities than the corresponding ones in the 2.2 plasmas.

The haptoglobin type is an individual characteristic and the differences between the three types appear to reflect real differences in the mode of formation of the specific group of macromolecules. Smithies and Walker (1955) showed that the differences were genetically determined, and put forward the hypothesis that they depend on a pair of allelic genes Hp^1 and Hp^2, type 1.1 individuals being homozygous for Hp^1, type 2.2 homozygous for Hp^2, and type 2.1 being the heterozygote Hp^1/Hp^2. This genetical hypothesis has been substantially confirmed by extensive family studies (Galatius-Jensen, 1957; Harris, Robson and Siniscalco, 1958).

The relative frequencies of the haptoglobin phenotypes have now been reported in several populations from North America and northern Europe, and also in two populations from Africa. The data are summarized in Table I. The outstanding point is that

Table I

REPORTED FREQUENCY OF HAPTOGLOBINS IN VARIOUS EUROPEAN AND AFRICAN POPULATIONS

Population	*Number tested*	*Haptoglobin type* 1.1		2.1		2.2		*No haptoglobin detected*		*Authors*
		No.	%	No.	%	No.	%	No.	%	
Canada	49	10	20·4	25	51·0	14	28·6	—	0·0	Smithies (1955)
U.S.A. (non-Negro)	54	6	11·1	29	53·7	19	35·2	—	0·0	Sutton *et al.* (1956)
Denmark	1033	175	16·9	483	46·8	375	36·3	—	0·0	Galatius-Jensen (1957)
England	218	22	10·1	121	55·5	69	31·7	6	2·7	Allison, Blumberg and ap Rees (1958)
England	114	20	17·6	55	48·2	39	34·2	—	0·0	Harris, Robson and Siniscalco (1958)
Basque	107	15	14·0	49	45·8	42	39·2	1	0·9	Allison, Blumberg and ap Rees (1958)
Liberia and Ivory Coast	142	69	48·6	60	42·3	13	9·1	—	0·0	Sutton *et al.* (1956)
Nigeria	99	53	53·6	11	11·1	3	3·0	32	32·3	Allison, Blumberg and ap Rees (1958)

the frequencies in the African populations are very different from those in the other populations studied. Thus the 1.1 phenotype occurs in about 50 per cent of both African series, but in only 10–20 per cent of any of the other populations. This result is of obvious anthropological interest.

The other point which has emerged from these studies is that, in some of the populations, individuals have been encountered with no detectable haptoglobins at all in their plasma. This phenomenon is rather uncommon among the European and North American populations studied, and was not observed in one of the African populations. Among the Nigerians examined by Allison, Blumberg and ap Rees (1958), however, no haptoglobins could be detected in some 32 per cent of the population.

The significance of this is at present rather obscure. It is probable that an "absence" of haptoglobins may occur for several different reasons. Thus, Laurell and Nyman (1957) have shown that if the circulation is flooded with haemoglobin, as may occur in an acute haemolytic disease, the plasma can be completely depleted of haptoglobins in twenty-four hours, and it is possible that chronic haemolysis may result in a similar phenomenon. On the other hand, occasional examples of such an "absence" of haptoglobins have also been observed in Europeans in which there was no evidence that any haemolytic process was going on, and it is likely that, at least in some cases, it may represent a genetically determined defect in haptoglobin synthesis rather than excessive rate of breakdown.

In view of all this it is of obvious interest to find out what the relative frequencies of the haptoglobin types are in other populations, and recently we have had the opportunity of collecting such data from several rather diverse localities in Italy. These include two small villages, Berra (in the Ferrara area) and Illorai (Sardinia), and two large towns, Naples and Catania (Sicily). The findings are shown in Table II.

The frequencies of the different phenotypes are very much the

Table II

FREQUENCY OF HAPTOGLOBIN TYPES IN FOUR ITALIAN POPULATIONS

Population	Number tested	*Haptoglobin types* 1.1 No.	1.1 %	2.1 No.	2.1 %	2.2 No.	2.2 %	*No haptoglobins detected* No.	No. %
Berra	120	20	16·7	57	47·5	42	35·0	1	0·8
Illorai	147	18	12·2	74	54·4	55	37·4	—	0·0
Naples	93	10	10·8	44	47·3	39	41·9	—	0·0
Catania	63	10	15·9	31	49·2	22	34·9	—	0·0

same in each of these population samples and, in fact, they do not differ significantly either from one another or from the frequencies reported from other European populations which have been examined (Table I). Three of the populations, those from Berra, Illorai and Catania, were of special interest because in each of these areas there is a relatively high incidence of thalassaemia. We had originally thought it possible that the relative frequencies of the haptoglobin types might be influenced in these populations because of differential selection by the presence of a high incidence of the type of mild chronic haemolytic anaemia which may be found in some of the thalassaemic heterozygotes. This, however, does not appear to be the case (Siniscalco *et al.*, 1958). It is also of interest in this connexion that, out of the 423 individuals tested in these four random samples, in only one instance could no haptoglobins be detected.

Direct estimation of the gene frequencies from the phenotype frequencies and calculations of the expected numbers of the three types in each population, assuming a Hardy-Weinberg equilibrium, show very good agreement between the observed and expected numbers in each case (Table III). This was also true of the other populations which have been studied, with the possible exception of the Nigerians investigated by Allison, Blumberg and ap Rees (1958), where the high incidence of individuals without detectable haptoglobins makes the interpretation difficult.

Table III

OBSERVED NUMBERS OF INDIVIDUALS WITH DIFFERENT HAPTOGLOBIN TYPES AND THE EXPECTED NUMBERS ASSUMING A HARDY-WEINBERG EQUILIBRIUM

	Haptoglobin types							
	1.1		2.1		2.2			
Population	Obs.	Exp.	Obs.	Exp.	Obs.	Exp.	χ^2	*p*
Berra	20	20·0	57	57·6	42	41·4	0·012	~0·9
Illorai	18	20·1	74	68·5	55	58·4	0·859	>0·3
Naples	10	10·8	44	41·7	39	40·5	0·242	>0·5
Catania	10	10·1	31	30·2	22	22·7	0·044	>0·8

The observation that the gene frequencies in each of these four Italian populations were not appreciably different from one another or from those described in any of the other European populations which have been studied (Table IV) is noteworthy.

Table IV

ESTIMATED GENE FREQUENCIES OF Hp^1 AND Hp^2 IN DIFFERENT POPULATIONS

The small proportion of individuals in these populations who had no detectable haptoglobins has been omitted from the calculation. This makes the assumption that they may be derived equally from any of the three types.

	Estimated gene frequency		
Population	Hp^1	Hp^2	*S.E.*
Canada	0·46	0·54	0·04
U.S.A. (non-Negro)	0·38	0·62	0·04
Denmark	0·40	0·60	0·01
England (Allison, Blumberg and ap Rees, 1958)	0·38	0·62	0·02
England (Harris, Robson and Siniscalco, 1958)	0·42	0·58	0·02
Basque	0·37	0·63	0·02
Italy:			
Berra	0·41	0·59	0·02
Illorai	0·37	0·63	0·02
Naples	0·34	0·66	0·02
Catania	0·40	0·60	0·03
Liberia and Ivory Coast	0·70	0·30	0·02

If the situation was one of neutral equilibrium with no selective differential, then we might have expected greater variations in gene frequencies to have been encountered, in consequence of genetical drift. This is particularly so in respect to the relatively small and isolated populations such as those from the villages of Berra and Illorai. It seems probable, therefore, that there may be a balanced polymorphism resulting in an equilibrium stabilized at these particular levels of gene frequencies. There is, however, as yet no direct evidence which could suggest in what way the three phenotypes may exhibit selective differences or why the heterozygotes should be at any advantage relative to the homozygotes. If such a balanced system exists then the selective forces operating must be common to, and of the same order of magnitude in, populations living in such widely different environments as those found, for instance, in Denmark and southern England on the one hand and Sardinia and Sicily on the other. At the same time the situation must evidently be very different in Africa.

Our knowledge of the population distribution of these genes is still, of course, very fragmentary. However, we may expect that when a more complete picture of the pattern of their frequency distribution has been built up, the results will be of considerable importance in helping us to understand the biological significance of this form of biochemical polymorphism in man, and no doubt they will also be of some interest when considered from the viewpoint of the anthropologist.

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Morganti: Dr. Siniscalco, have you any more information about the African Negro types?

Siniscalco: The data on Negroes that I presented were collected by Neel, Sutton and co-workers in the U.S.A. (Sutton *et al.*, 1956, *loc. cit.*) and Allison and co-workers in England (Allison, Blumberg, and ap Rees, 1958, *loc. cit.*).

Barnicot: Neel and his group (1958, *Hum. Biol.*, **30**, 43) showed in Liberian samples that the frequency of sickle cell was about 8 per cent, which is considerably lower than most of the figures for the west coast of Africa. This is another respect in which the Liberians may differ.

Mourant: It is curious that many Africans seem to lack any detectable gene of certain systems that we have studied in Europe. There are many Africans who have neither the blood group gene Fy^a nor Fy^b, others neither S nor *s*, or neither haptoglobin *I* nor *II*. One wonders whether, if genetics had been started in Africa, many genes might have been found that we have not yet discovered; then one would say how odd the Europeans were, because they lacked so many!

Harris: There is another group, β-globulin *D*, where the Africans seem to have 3 or 4 per cent, and the Europeans have none at all.

Mourant: We have studied a couple of blood group genes which occur in Africans but do not seem to occur in Europeans.

Morganti: Is it possible that the alteration of proteid components, which is supposed to be common in Africans, might alter the haptoglobin pattern?

Siniscalco: They might be altered quite suddenly from a quantitative point of view but this does not modify the phenotype. The only thing as far as we know that could alter or, to be more precise, could conceal the phenotype would be haemolysis; the occurrence of even slight haemolysis *in vivo* might remove the haptoglobins from the plasma, but only temporarily. However, it is improbable that haemolysis could cause confusion between different phenotypes, as long as haptoglobin remains detectable.

Lehmann: Does not haptoglobin play a part in binding haemolysed haemoglobin? If you carry out determinations two weeks after haemolysis has occurred, is it not likely that the haptoglobin has been used up and you will not detect any? It appears that the African results should be repeated carefully.

Harris: Where the experiments are done by infusing haemoglobin, the plasma can be completely depleted of haemoglobin within 24 hours. Evidently they return within a few days, and if there is an untreated pernicious anaemia with haemolysis, there is no haptoglobin; but once treatment is started, within a few days it reappears. It is difficult to believe that there is sufficient chronic haemolysis going on which would lead to these results. Certainly, all this work needs to be repeated.

Lehmann: Could chronic malaria cause the picture seen in your Table IV?

Harris: It could do; one does not know. But whatever the cause of the absence of haptoglobin, there are big differences in frequencies of the haptoglobin types which one can detect in Africans and in Europeans. On the other hand, within Europe—even in those rather isolated villages in Italy—the frequencies are extraordinarily similar to one another.

Barnicot: This might be an opportunity to test Africans in London. Even if you got only a small sample from these Africans in London, it might perhaps be an effective control.

Harris: This is obviously what has to be done; and also the samples will have to be taken under more controlled conditions.

Mourant: It would be interesting to get some data on eastern European populations in view of the slight suggestion that the Basques have a lower frequency of haptoglobin *I* than other western Europeans. Since we now have three genes, for all of which the Basques represent one pole and the eastern Europeans the other, it may be that we should find a higher frequency of haptoglobin *I* in eastern Europeans. One might also test the Ligurians, if one can identify them sufficiently precisely among the present peoples of Italy.

Lehmann: In a slide* showing a child with sickle cell anaemia, you can see what is called the "hot-cross bun" feature of the skull. It is due to a swelling of the top bones of the skull which are usually flat. Usually

[*Not submitted for publication.—Eds.]

red cells live for 100–120 days; in these children they live only from 60–70 days. Therefore, the bone marrow which has to replace the cells is working overtime so that in these children the bones which, in normal human beings, have no blood-producing marrow, do possess blood-producing marrow and that gives their skulls this peculiar shape.

An X-ray picture shows atrophy of the base of the skull, and peripherally—centrifugally—there is much proliferation of new delicate bone. This causes a "hair on end" appearance.

You get the same features in thalassaemia; you get the appearance of a skull in which the patient produces more blood than normally and in bones which usually are inert. With other haemolytic anaemias, e.g. malaria, syphilis, etc. you may get this alone. Certainly in long bones there are some changes, which are rather more typical for sickle cell anaemia than for any other haemolytic condition; because in sickle cell anaemia you also have infarcts, which give the bone some peculiar disturbances in function, resembling those seen in Perthes' disease.

Brothwell: What does the parietal look like in the dry specimen? Do you get a lot of osteoporotic pitting?

Lehmann: Yes, you do.

Brothwell: Is it just the same as in normal osteoporosis?

Lehmann: In normal osteoporosis the skull is not thickened. That is due to an absence of matrix, due to malnutrition or hormonal disturbances. Here it is absence of bone because of replacement by bone marrow. You may get the same picture in tumour formation.

Brothwell: Could the archaeologist pick it out as distinct from ordinary osteroporosis?

Lehmann: Yes.

Cook: With a sufficient number of bones one should be able to tell when a district became malarial?

Lehmann: We can tell when the district had an increased incidence of haemolysis in early youth. That does not mean the same thing, though at the present time most of the blood destruction is due to malaria in that area. But there may be other diseases; they may be inherited defects of blood cell production such as thalassaemia. Dr. Oakley, would you say something about the Piltdown skull?

Oakley: We are interested in the histology of the so-called Piltdown

cranium, because it shows great thickening of the diploic tissue, and the outer and inner tables are quite thin. The whole of the thickening is due to the expansion of the diploic tissue which is spongy; about twice the normal thickness of the wall of the skull. We think it was chosen for the fraud because of the abnormal condition, which it was hoped would be mistaken (as it *was* mistaken) for some extinct type of man. We have reason to believe that this skull might have been derived from a Mediterranean necropolis. Some of the other specimens, fed into the site for the purpose of the hoax, are of Mediterranean origin. I have been hoping to get, from the pathologists working in the Mediterranean area, some clue which might help us in confirming this origin. It does not seem to be a case of osteoporosis, because that does not go with great thickening, as Dr. Lehmann has said; nor is the outer table perforated. That is quite intact, but the internal diploic tissue has a very spongy texture. Have any of our Italian colleagues come across skulls that show that feature?

Brothwell: Surely, very little is known on the variation in thickness of skulls, including the variability in the proportion. There seems to be quite a variation of diploe to outer tables, judging by collections in various British museums.

Oakley: Yes, I think we are quite ignorant in the matter; that is why I want any facts that have been brought to light.

Barnicot: In connexion with the Swanscombe skull some data was given about normal variation in thickness; that was a crucial point about it.

Oakley: Thickening, as such, may not be significant—you get tremendous variations, probably up to the Piltdown thickness, in almost any large collection of human skulls (ancient or modern). That is not really the point.

The Piltdown condition is that the outer and inner tables are extremely thin, while the diploic tissue is greatly expanded and uniformly spongy. It is the enormous thickening of the diploic tissue which is so characteristic in the Piltdown skull; whereas in the Swanscombe skull, which approaches, but does not equal, it in thickness, there are relatively thick outer and inner tables. I have seen many abnormally thick skulls, but in our collection I know of only one other skull where all the excessive thickness is due to diploic spongy tissue, and that is from

Tierra del Fuego. I should like to know whether in the Mediterranean region such a condition of the skull is a reflection of some chronic anaemia?

Lehmann: I would certainly say so. After having given due warning that we should not immediately interpret as thalassaemia the thickening in skulls like that, I must admit that in practice we do diagnose thalassaemia on such findings. But we cannot be too careful, because there can be other causes.

Kirk: It might be of interest to the archaeologists if they could notice this thickening, or if the specimen they sent to the osteometrists showed it. It might imply, if it seemed to happen at a particular stage in a particular community, that the community was overtaken by something like malaria, or possibly syphilis; and this might be significant for their cultural history.

Lehmann: Mr. Cook has already asked whether it suddenly appears in populations. Malaria could, but thalassaemia would not appear in a large number suddenly unless there had been an immigration of a new population. Thalassaemia is found more in young children; but if much thalassaemia appears suddenly in a grave it might indicate that many people had been killed off by an epidemic.

Ceppellini: The ideal would be to have a number of skulls, 2000 years old, of a given population with very little thalassaemia and then another collection of skulls, possibly dated 1000 years later, with plenty of thalassaemia, in which case we could say that malaria appeared between 2000 and 1000 years ago. It is quite unrealistic to hope for such ideal conditions, but it would be very exciting.

Siniscalco: It would be quite difficult to discriminate between malaria and thalassaemia if they give the same kind of alterations. You would notice the many people affected by this bone alteration because of malaria, and you would not notice the 5 to 10 per cent of thalassaemic cases.

Ceppellini: But what about the change before and after at different ages?

Harris: If you do the experiment the other way round e.g. if, in an area like Ferrara or Sardinia, you pick some skulls which are 50 or 100 years old, you would know that here you have a population where there was thalassaemia, and where you know that there was malaria.

You have other populations elsewhere, e.g. perhaps around Rome, where you know that there is a lot of malaria and possibly not so much thalassaemia. Presumably you could get samples of skulls of fairly recent vintage, of the last 150 years, and find out how frequently this occurs.

Ceppellini: There are ossuaries which are dated.

Lehmann: That could be one of the practical suggestions which we are aiming to reach today: to examine children's skeletons to see if they are any different e.g. in Sardinia and in Rome. If that works out it might be used for prehistoric diagnosis.

Strong: Can one describe what happens to the bone after it was put into the ground; are there no important changes?

Lehmann: You would not get this change; there are usually changes in other directions.

Mourant: There is hardly ever any difficulty in distinguishing between a postmortem and an antemortem change.

Brothwell: Dr. Siniscalco mentioned the difficulties in studying the inheritance of such factors as bone form and size. I wonder whether certain features, such as presence or absence of wormian bones, metopic sutures and other such non-metrical characters might be useful in tracing the origins of people such as the Etruscans. Admittedly, these may have quite a complex genetical basis. However, Jørgensen has already undertaken a study of the metopic suture, (Laughlin, W. S., and Jørgensen, J. B. (1956). *Acta genet. (Basel)*, 6, 3) and after a series of radiographic examinations, concludes that the retention of this suture may be fairly simply inherited. Could this, then, be a possible line of research to be applied to Italian skeletal remains? Laughlin and Jørgensen, in their work on the Greenland Eskimo, have found that they can differentiate populations on certain of these discontinuous morphological traits. Such an enquiry might be used in collaboration with material and serological studies in solving ethno-Etruscan problems.

Siniscalco: Certainly it would be worth while following this line of research but I think that I have produced one more reason why osteometric data should be treated with caution. As far as the sutures are concerned, they also represent most probably a quantitative character. Extensive family data would certainly be very valuable in this respect.

Brothwell: Yes; fortunately the retention of the metopic suture into adulthood, or the presence of wormian bones, can be observed radiographically. Family studies would certainly help in assessing the value of frequency variations of these features in earlier populations.

Mourant: What was the age of the Etruscans when they died? Is there any considerable number of bodies with the age written on the tombstone?

Ward Perkins: You will get very little of that before the Roman period, and then only under certain fortunate conditions. In the Roman period, yes; under certain conditions one will get individual sarcophagi with individual indications of age.

Foti: There are some Etruscan sarcophagi at Viterbo with indications of age but they were opened by inexpert people who were not interested in preserving the bones that belonged to the date. Two were found in April 1958, dated about 200 B.C.

Banti: We have found something quite extraordinary from about 300 B.C. We found urns and sarcophagi which had already been discovered in ancient times, and no longer contained bones, or contained only a mixture of material. In order to have something that might be useful in establishing the age, we need to find a sarcophagus which has not been touched by vandals; because even if they had left the bones, we would never know whether or not they had added something else to the tombs. The vandals took out all they wanted, mostly jewellery and beautiful vases; but they often put into the tombs something which came from other tombs. Probably they never put bones into them, but they might have taken bones up with other material. It is not very easy to find a sarcophagus intact. We only find sarcophagi with the age inscribed in southern Etruria, because in northern Etruria only incineration was practised. From later times we found inscriptions giving the date of death. Sometimes, too, we cannot rely on the inscribed date, because the sarcophagus had been re-used.

Ward Perkins: From the archaeological point of view, it is obviously premature to look for concrete historical results at this stage, particularly in a case as complicated as that of the Etruscans, where there are so many disturbing factors. On the other hand, this symposium has been extremely useful in showing something of the methods and objectives of our various specialized studies and, perhaps equally important, something of the limitations of each. The positive conclusion which emerges is that both the archaeologist and the geneticist, potentially, have material to furnish to the other side, which may eventually be useful. We dig up bones; we certainly cannot evaluate them in historical terms, but they are just as much a part of the material evidence we dig up as the pots that we find, or the constitution of the metal objects; and it is our business to be aware of that fact. So far as it is possible, we should preserve the material in its archaeological context. In precisely the same terms, we are in a position to furnish to the geneticists material which, when a sufficient body of it has been accumulated to justify statistical results, should be of great help to them in giving a chronological perspective.

With the practical end in view of formulating some scheme for producing this material and for passing it on to those who wish to use it, I would suggest to my Italian colleagues who are present, that in the near future it would be extremely useful if we could report on the results of this meeting to those persons in Italy who are best in a position to help in the matter; and we should be able to state in clear and precise terms what we ask of them. We should summon a meeting to which persons and institutions working in this particular field should be invited, to hear something about this symposium and to formulate a practical scheme for providing the material required. The obvious bodies which should be invited to send representatives would be, first, the Superintendencies from North and South Etruria; secondly, the Istituto di Studi Etruschi; thirdly, those of the foreign academies in Rome who are interested in this matter; and, if it were possible to persuade one of our medical colleagues to come and put

the whole thing in words of one syllable, it would be ideal to have one of them too.

I should be extremely happy to organize such a meeting and to house it in the British School, if that were of any help. It might be a practical advantage from the point of view of the bilateral Anglo-Italian contact which we formed at this meeting. At that meeting, in addition to reporting in practical terms on the results of this meeting, and stating our very clear requirements, it would be important to see what can be done to furnish the material both to our English and to our Italian medical colleagues. The whole value of this meeting will be lost unless we move forward on some unified and agreed basis.

Foti: I agree perfectly with Mr. Ward Perkins' remarks. It would be most appropriate as one result of our meeting here to take the necessary measures to use the information we have gathered here; also to communicate our findings to the authorities in view of the planning of future programmes. It is an excellent idea that some kind of a meeting should be organized.*

Barnett: One point which should not be overlooked is that if you are trying to establish ethnic relationship between the Etruscans and other races with whom they are, according to some literary sources, connected; if you are making plans to collect more accurate data about the Etruscan remains in Italy, it is logical, if not indispensable, to make sure that similar data for comparative purposes are obtained from the alleged original home of the Etruscans, in Asia Minor. Already there are reports that the Americans are proposing to reopen excavations at Sardis, the capital of Lydia, and another American body has been for some time excavating Gordion, while the German Archaeological Institute at Istanbul has been excavating for some time at Boghaz-Köy. It would be unprofitable to work in isolation instead of co-ordinating our efforts with Turkish workers who may be interested, or may be induced to be interested, and particularly the Americans working in Asia Minor. Any future work should be planned in co-operation with those persons.

* The meeting proposed here was held on the 3rd June, 1958, in the British School at Rome, with representatives of the two Superintendencies of Antiquities, for Northern and Southern Lazio, the Istituto di Studi Etruschi ed Italici, and the French, Swedish and American Schools attending.—Eds.

Strong: In view of the positive request by the British Museum of Natural History for specimens, could we not promise now that the delegates would try to present that material to the British Museum as soon as possible?

Cook: We must first decide how many places and bodies wish to examine these bones. Naturally, the British Museum of Natural History would wish to have a look, but so no doubt will some of our Italian colleagues. We ought to discuss what would be the best way of collecting and examining this material.

Morganti: It would be advisable to choose the right femur for present research, and reserve the left one for future study.

Piganiol: We must not, however, confine ourselves to the Etruscans in this matter. I don't suggest that we should keep all the bones found, but we should keep all those which are dated. We should extend our field of co-operative work beyond the limits of the Etruscan problem, not excluding the other ancient peoples whose traces are found, for instance, in Bologna, in Tuscany and in Liguria. After all, the Superintendencies of archaeological departments have wider interests than that of the Etruscan field alone.

Ciotti: In fact, it is by comparing these materials that we may get some positive information about one particular field.

Piganiol: We may wish to know, for instance, what was happening on the other side of the Adriatic during the period in which we are interested. We cannot separate the various subjects. We should ask that all available archaeological material should be furnished to us for study—how could one choose between this material, how could any Superintendency know exactly which material we want?

But the most important point is to keep the skeletons which are dated, and it would also be helpful to have some idea of the site where the bones were found.

Brothwell: Prof. Neppi Mòdona, can you tell us about any programme of future work on Etruscan skeletal material which may be carried out by German anthropologists?

Neppi Mòdona: I know of only one German working on this subject —I do not think that particular line of research is being followed in Germany at present.

The Naturalistic Section of the Institute of Etruscan and Italic

Studies in Florence prepared a questionnaire which was sent to all the Directors of Museums, within recent years, and quite a lot of information has been collected in this way. In Italy, there is a Committee to study the practical problems of Etruscan remains, but so far no conclusions have been reached. At the University of Padua some accurate studies have been carried out on material from Belverde on Mount Cetona near Chiusi, dating back to the Bronze Age, but although useful they are not closely linked to the Etruscan period. The University of Padua is about to publish the results of some important work on the material collected, and something will probably be shortly published in a new issue of *Studi Etruschi*.

However, they had not thought of the possibility of co-operation with the biologists, and this should provide an extremely useful new approach to future research. A concrete proposal should be made at this meeting and submitted to our Institutes; also the existing Committee of the Institute of Etruscan and Italic Studies should be informed so that contact could be established with interested people, on a practical basis.

Ciotti: We are faced with two problems. (1) We should devise a general memorandum specifying some broad outlines for any kind of archaeological work being carried out anywhere.* As Prof. Piganiol has emphasized, we must consider not only the field of Etruscology. (2) The financial aspect of making an experimental trial in a particular area is beyond the scope of usual archaeological methods.

Ward Perkins: Two working generalizations can be made. One is that the results achieved at a conference are usually in inverse proportion to the number of delegates; the other is that the results of a programme are likely to be in direct proportion to the interest of the individuals who are prepared to work on it. Here, we are in a fortunate position in that we are a small group, and in that we have present several people who have expressed their interest and anxiety to help. We have Prof. Neppi Mòdona who has very kindly said that the Institute of Etruscan and Italic Studies will throw its weight into anything we choose to propose. We also have representatives from the Superintendency of Southern Etruria. We should try to work out a practical plan for the area which we have been discussing and in which

* See appendices I and II, pp. 246–249.

we are directly interested, and leave the door wide open for informing everybody else, thereby hoping to get the collaboration of our colleagues in the other Superintendencies of Italy, Asia Minor and elsewhere. Results will be best achieved by not trying to aim too high at the outset. We should try to put this matter into reasonably concrete, practical and relatively modest terms for the moment.

Banti: We should limit the area of our research to Tuscany and the neighbouring regions. Study of the whole of Italy and the Orient is something that must be left for the future. If, by concentrating on Etruria and the neighbouring regions, we succeeded in finding some differences between them, then we could consider the problem of origins which would lead to study in other places, e.g. Asia Minor, Mesopotamia.

Ceppellini: We are dealing with two distinct problems: (1) the study of skeletal remains, and (2) the study of living populations. For both these problems we may refer to blood group determination as a biological tool for ethnological investigations.

As far as the survey of living populations is concerned, it would be possible to begin this now in Italy, as a result of the recent formation of the National Committee for the Study of Blood Groups in Italy. Therefore, all we need to do in this respect is to invite some workers in other disciplines—archaeologists and glottologists—to take part in the planning and development of the investigations.

As regards the study of skeletal remains, the chief difficulty is that there has not yet been devised a satisfactory method for blood group determination on old bones. Nevertheless, since our colleagues at the British Museum (Natural History) are ready to undertake the huge task of blood group research into old bones, and especially Etruscan bones, we Italians should provide them with some of this material, and hopefully await the results of their research.

Furthermore, it would be possible, with very little effort, to store the vast majority of the material collected in the meantime, probably in a deep-freeze. In this way we should possess a "bone bank", and could be ready to join in the investigation immediately a good method was available.

Another possible approach to the study of skeletal remains is that

of skeletal measurement, and on this matter we should hear the opinion of the anthropologists.

Ciotti: I agree that we should take steps to preserve the material and this work must be organized on a scientific basis with the advice of the anthropologists.

Barnicot: Mr. Brothwell and I could prepare a short memorandum on the osteometric requirements relating to this question.*

Ciotti: This memorandum should go with the report which will be prepared for the International Archaeological Congress, and it could also be circulated to the Superintendencies, because this would make more authorities aware of our requirements. These in turn can pass the information to their departments which are doing this work.

Ceppellini: I would suggest that, for instance, when you are excavating in Perugia you could hand over all the bones which you find to the Institute of Anatomy of the University of Perugia, instead of sending them to a cemetery. The matter is not such a simple one. If we must waste the material, that is one question; if it is to be preserved for future study, that is another.

Ciotti: We would not bury the bones in a cemetery, in any case, or waste them. But the question of packaging and transport is important. A skeleton which has been buried a long time is quite fragile. Should we first photograph and measure the bones? What soil specimens and general indications as to the site where they were found would be required?

Foti: For every skeleton which is excavated by our group, we make out a card indicating the date, site, etc. Then the problem arises: who will collect these cards? If sent to an Institute of Anatomy they might be thrown away there.

Ceppellini: They should be sent to some Institute with an interest in the subject, and then they would not be thrown away.

Bolelli: There appear to be two distinct problems: that concerning the living and that concerning the dead. As far as the living are concerned, they can be studied chiefly by the biologists. For the dead, we are personally responsible, and it is our duty to circulate the information which we have gathered here to our colleagues and to experts in the fields of archaeology and glottology. This would not be difficult.

* See appendix II, pp. 248–249.

In fact, I am already looking forward to going home and inviting my friends and colleagues, anthropologists, biologists and philologists, to meet at my house and talk about what we have learned here, particularly about the preservation of old bones which may prove to be valuable from many points of view and which have so far been rather neglected.

Ciotti: We must not exaggerate the question of wasting or neglecting old bones. In fact, we have measured them on many occasions, we have photographed them and stored many of them; but we have by no means done this routinely, and we now need to establish this kind of research on an organized basis, and I think we have already agreed on what is to be done.

Neppi Mòdona: It would, of course, be untrue to say that nothing had been done, but it is true that only very little has so far been done. As I mentioned before, it seems unwise to rely on the material which has already been collected. Since people were not aware of the potential value of this material, the dating and filing were not very accurate, and the use of this material for present studies might only lead to confusion. For instance, in museums a record is kept of everything collected, but this is done only from the point of view of cataloguing the contents of the museum, and not scientifically. As I told you, we have obtained not a great deal, but some, information in reply to the questionnaire circulated to museums, and I should be glad to make this information available to anyone who is interested in it.

Kirk: On this detail of how to transport bones: there seems to be a difference in requirements between the osteologists, on the one hand, who want whole skulls, and the serologists, on the other, who only need the tops of femora. It would be ideal if the excavators could deal with both skulls and femora, but, if conditions are difficult and time and money scarce, it would be quite simple to collect the tops of a number of left or right femora and send them in polythene bags to a collecting centre, like the British School at Rome, for despatch to the serologists who are interested.

Oakley: Speaking on behalf of the British Museum (Natural History) I am very grateful to Mr. Kirk for putting that point of view. We do not want to cut across any more ambitious programme of investi-

gation of the skeletons in Italy, either oestometric or serological. We can make provision for later osteometric work, since the head of the femur could be repaired after drilling, and returned ready for restoration to the shaft, if that were indicated as a requirement. Moreover, since we would be using less than 1/50th—probably less than 1/100th—part of the skeleton, we would not preclude any much fuller serological investigation that might be made by our Italian colleagues. We would simply like to make a preliminary attack on this problem as soon as possible to see what results can be obtained. In the light of that, perhaps a more ambitious programme could be built up.

Barnett: The Natural History Section of the British Museum should draw up a form of standardized questionnaire or instructions, which should be sent to the excavators in Turkey, particularly as it seems that, as a result of this conference, Italy will be well looked after.

I disagree with Prof. Banti's approach—which appears to be superficially convincing—that we should deal first with the Italian question and then after five or ten years proceed with the others. Meanwhile a very large number of skeletons may have been excavated in Turkey and treated chemically in a way which will render them valueless, or they may be lost to science through lack of opportunity for study, when work along the right lines might be proceeding simultaneously. When material is excavated in the immediate future we do not want to see it dispersed or mishandled while it is still fresh. Then the work would have to be repeated when perhaps these particular cemeteries where the material was discovered are no longer available.

Hencken: In Ankara there is a very good physical anthropologist who handles the bulk of this material.

Cook: I am sure that only good can come of informing other workers.

Mourant: Concerning the point of using a deep-freeze: it is a very good ultimate ideal, and the sooner the better. We have a cold room for blood grouping sera—something like 500 cubic feet—which cost about £2000 ten years ago and it has saved its value many times over.

Barnicot: Do you think that, in the case of these bones, we might save a little money by keeping them in desiccators instead of in a deep-freeze?

Mourant: Some sort of desiccation process may be most useful and may save the expense of deep-freezing, but it is not quite certain that all biological specimens retain their specificity when freeze-dried.

Ceppellini: The use of desiccators for large quantities is not a simple matter.

Harris: The simplest is to use a polythene bag which excludes air.

Mourant: I doubt if a polythene bag is sufficiently impermeable. But if bones are kept reasonably cool in a polythene bag, and if they have already been exposed to air and moisture for thousands of years, the biological properties are not likely to change much in a few years. It is a different matter when we expose material which has been naturally preserved for ages by freezing or by its chemical environment. Here we must be much more cautious.

Morganti: At the moment, we are not ready to go ahead with a particular programme of blood group determination on old bones. Therefore, we are grateful that the Natural History Section of the British Museum is willing to make a start. Later on, others may wish to follow them, and the necessary measures to facilitate other people in similar research can then be taken, but for the present we can leave this work to our colleagues at the British Museum.

Ceppellini: From the scientific point of view we are very pleased to collaborate with the British Museum in this, which is a specialized research. For the time being, it is clear that we can help only by sending them material. If they achieve some concrete results, then Italian centres will be anxious to join in this huge programme of typing skeletal remains.

As regards the study of living populations, also, we should very much like to organize this on some international basis. The collaboration of Dr. Mourant, who is the father of genetical anthropology as far as blood group determination is concerned, would be very valuable. Furthermore, our greatest need is not for financial but for moral support, and in this the Ciba Foundation could help us.

Cook: Dr. Wolstenholme has suggested that if the work of collecting and packaging bones does involve expense, it is possible that the Ciba Foundation might have funds at its disposal to help in this; and secondly, that if this inquiry should be leading to useful results, then it might be desirable to hold a similar symposium in two or three

years time, to discuss results and to see what further work could be done.

Wolstenholme: If anybody does require financial help for packaging and transport, etc., the applications should be canalized in some way. If somebody, for instance, Mr. Ward Perkins, would be prepared to be the person in Italy to whom people could make inquiries as to financial support, and would pass them on to us, we would do our best to find some way of helping the more necessitous cases.

Cook: This symposium has been, in the main, a success and is a pattern of the type of meeting that is badly needed. The scientists have shown a remarkably good disposition towards their humane colleagues, and a very creditable interest in their problems. The archaeologists too have become more aware than they were before that not only can they learn much from the scientists, but they can also offer a great deal to them. The test of this meeting will come in the next two or three years as we begin to see what material is produced and what is made of it.

For all members of the symposium, it has been a great pleasure to meet together and for me to meet each of them. We feel very grateful to the Ciba Foundation which has sponsored this meeting, and has also provided for us in such a comfortable and hospitable way. I think that, as much as anything, should spur us on to produce the kind of results that will justify another meeting like this.

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APPENDIX I

MEMORANDUM ON BLOOD GROUPING: REQUIREMENTS OF BRITISH MUSEUM (NATURAL HISTORY)

K. OAKLEY and MADELEINE SMITH

Anthropology Section, British Museum (Natural History), London

The British Museum (Natural History) is prepared to test series of human bone samples from current and future excavations at early Etruscan* and/or Roman sites. For this project *at least* 50 samples of each group would be needed. (100 samples of each group would be preferable.)

Each sample should represent one individual. The simplest way of meeting the minimum requirements would be to conserve the upper parts of femora (viz. head, neck and the top of the shaft), as this portion would contain the right quantity and quality of material for testing. It is important statistically that no individual should be represented by more than one sample. Therefore where skeletons are fragmentary, and where the remains of several may have become mixed, it is essential to collect heads of only right or only left femora.

It would not matter if the samples in either the Etruscan or Roman series were collected from a number of different sites; provided that a sample of soil and a note of the local environment of the specimen is taken.

It is suggested that the bone samples should be collected in small polythene bags, sealed with adhesive tape or in some similar way at the time of collecting. No kind of preservative should be applied to the bone sample.

The Director of the British School of Archaeology in Rome has agreed (subject to the approval of the Italian authorities) to assemble

* Before 390 B.C.

the collected samples, and to forward them in due course to the British Museum (Natural History).

If whole skeletons are being preserved, a note should be made, so that the heads of femora can be recovered after drilling for analysis. Otherwise residues of material will be preserved so that they can be retested at a later date by improved techniques such as those outlined by Professor Ceppellini (this symposium, p. 159).

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APPENDIX II

MEMORANDUM ON FURTHER OSTEOMETRIC STUDIES RELATING TO THE ORIGIN OF THE ETRUSCANS

N. A. Barnicot and D. R. Brothwell

Department of Anthropology, University College, London

(1) We should like to know firstly whether archaeologists working on Etruscan problems are anxious for further osteometric work to be done. We have presented a preliminary survey of the existing data at this symposium (pp. 131 *seq.*). On the whole we feel the evidence is against the Etruscans being a clearly differentiated physical group, and though there are certainly gaps in our present knowledge, we should not be inclined to pursue the matter for our own purposes. However, if archaeologists feel that further, more systematic, work would be valuable, we shall be pleased to do what we can to help. We therefore make the following suggestions.

(2) We suggest that the minimum number of bones to be kept should be:

(*a*) The skull if reasonably complete (including lower jaw);
(*b*) The femora and humeri (at least one of each; preferably right);
(*c*) The pelvis (or at least one side of it).

(3) We should aim at accumulating these remains from 100 males; this means that about 200 individuals would have to be collected and stored if sexing were going to be done later in the laboratory.

(4) Consideration must be given to the question of sampling.

(*a*) The material must be from sites which can be regarded as characteristically Etruscan from the population point of view.

(*b*) They should, if possible, represent the total population and not merely some special section of the community. It follows that precise notes on the circumstances of the finds are important.

(5) As regards organization, we suggest that the material should be accumulated at some convenient centre in Italy and that one Italian worker in this field and one English worker should be given facilities to examine and measure the material annually, working together.

(6) As regards material for comparison, we suggest the following:

(*a*) Roman remains from the Republican period.
(*b*) Bronze Age or Neolithic material from Etruria or elsewhere in Italy.
(*c*) Recent remains (i.e. last five hundred years) from Etruria.

(7) Any bones thought to show signs of disease should be retained.

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